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IN

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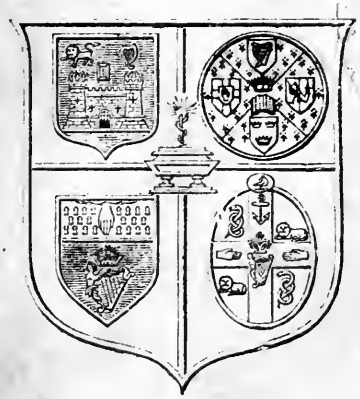
Edited by J. W. MOORE, B.A., M.D., Univ. Dubl.; F.R.C.P.I.; &c.



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5. Alkarnose. Von Dr. Freudenberg. Leipzig. 1899. Reprint. Pp. 4.

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THE DUBLIN JOURNAL

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JUNE 1, 1899.

PART I.

ORIGINAL COMMUNICATIONS.

ART. XIX.—*Further Observations on Uterine Cancer and its Treatment.*[a] By THOMAS MORE MADDEN, M.D., F.R.C.S. Ed., M.A.O. (*Hon. Causâ*), Royal University, Ireland; Obstetric Physician and Gynæcologist, Mater Misericordiæ Hospital, Dublin, &c., &c.

THERE can, unfortunately, be no question of the increasing frequency of cancerous disorders, including those of the uterus, as evidenced by recent reports of the Registrar-General. Nor is there less doubt of the wide differences of opinion still existing with regard to the respective value of the various methods of treatment available in cases of malignant diseases of the uterus. I, therefore, venture to bring the latter subject again before the Gynæcological Section of the Academy, in the hope that even so imperfect a recapitulation of my earlier and more recent observations may possibly elicit the opinions of others more competent than myself to discuss the gravest and not least frequent of the maladies peculiar to women.

Whether the views expressed in this paper are likely to be confirmed or corrected it is not for me to anticipate. In either case I shall content myself with the belief that they are not altogether devoid of clinical foundation, being based

[a] Read before the Obstetrical Section of the Royal Academy of Medicine in Ireland, on Friday, April 21, 1899.

on observations extending over a period of twenty-five years in the gynæcological department of the Mater Misericordiæ Hospital, Dublin.

Within the last six years, from November, 1892 to 1898, inclusive, 1,056 gynæcological cases came under notice at the hospital, and of these 31 were cases of uterine cancer, and 12 of vulvar or vaginal malignant disease—that is to say, the proportion of such cases to all others was, roughly speaking, 3 per cent. of the former, and 1 in 88 of the latter. In the preceding five years there were somewhat less cases of uterine cancer—viz., 23—and 10 cases of vulval or vaginal malignant disease noted.

Moreover, although I cannot at present tabulate my earlier experience, I have sufficient evidence in my former notes of a large number of cases of uterine cancer to justify the conclusion that the proportion of such cases observed in the hospital during my first fourteen years' service there, although somewhat smaller than that recorded in the more recent periods, were it to be tabulated, would include considerably upwards of a hundred cases of uterine carcinoma within that twenty-five years referred to.

Confining myself, however, to the cases of this kind noted in the last eleven years, I may observe that the general origin of uterine cancer in the cervix was, I think, shown in 47 out of these 54 instances. In them, also, its probable connexion with cervical lacerations, in a majority of cases, was evinced by its occurrence in 39 women who had borne children, and in only 15 nulliparæ or unmarried women. Whilst of the other predisposing causes of carcinoma uteri, that of age was proved by the fact that 36 of these patients were approximating to or shortly beyond the menopause—*i.e.*, from 40 to 50 years of age: 10 were beyond the latter age; 8 were under 40, and of these one instance of medullary cancer of the uterus occurred in a girl only sixteen years of age.

In these observations, dealing with this subject as I now do from a clinical standpoint, under the heading cancer or carcinoma of the uterus are included not only epithelioma, medullary, encephaloid, or soft cancer, and the more exceptional scirrhus or hard carcinoma, but also sarcomatous

growths within the connective tissue of the uterus, and the other malignant conditions that are clinically though not histologically inseparable.

Hence, I must here group together all those pathological conditions of the uterine tract characterised by the malignant development of infective cells, by the local proliferation or constitutional effects of which (unless the result be obviated by timely and complete removal of the primary disease, or by some intercurrent fatality) death must eventually and inevitably be occasioned.

Early Diagnosis and Symptoms of Uterine Cancer.—I may remark that my chief purpose in this paper is to re-urge the importance of early recognition and prompt treatment of the first evidences of cervical carcinoma, inasmuch as I hold that once the disease has extended upwards so as to implicate extensively the superior uterine zones, or when it has originated in and largely disorganised the latter, any treatments proposed, including hysterectomy, can as a rule be regarded as merely more or less effective palliative measures.

Of the ordinary symptoms by which in the cases referred to patients were induced to apply for the gynæcological examination, by which the nature of their disease was disclosed, the most important were hæmorrhagic or other abnormal vaginal discharges to which, though generally at a later period, uterine or peri-uterine pain were commonly added.

Amongst these earlier symptoms one of the most frequent and constant was hæmorrhagic uterine discharge unconnected with other recognisable causes. This, more especially when occurring after the climacteric period, should arouse suspicion of its probable source in malignant disease, and always demands that careful local investigation by which its origin may be traced.

Vaginal Discharge in Cancerous Cases.—Another generally early and characteristic symptom of malignant disease of the uterus, and particularly of its medullary forms, is a thin albuminoid exudation from the vagina, occasionally blood-stained, but more commonly of a light greenish colour, and offensive in smell from the first. This, as the malady progresses, gradually becomes more excessive in quantity as

well as more ichorous, blood-stained, or muco-purulent, and, above all, assumes a special and almost pathognomonic fœtor, distinct from the fœtidity frequently present in other gynæcological cases where local cleanliness is neglected. By this irritant exudation the external parts in cases of advanced cancer are liable to become excoriated, and the most intense pruritus occasioned. Whilst at the same time, by the abominable and far-penetrating odour of that profuse albuminoid cancerous discharge by which her genitalia are bathed, and her linen stiffened as though with putrescent starch, the wretched patient's life is rendered a misery to those about her as well as to herself. This trouble is, however, one which may generally be effectually relieved by the means to which I must again refer.

Pain.—The extent and character of the pain complained of in cases of uterine cancer is chiefly dependent on the locality as well as the special form of the disease in each instance, being most intense where the fundus or body are implicated, and least experienced where the disease is in vaginal portion of the cervix, more especially in the more common epitheliomatous and medullary growths. In such cases the cervix may occasionally be destroyed by the former, or the vagina filled by the latter, without the occurrence of any very acute suffering.

In few of the instances of uterine cancer that have come before us was pain, one of the earlier of the symptoms by which the patient's attention was called to her disease. But generally, as the disease progresses, and more especially in its scirrhus form, the so-called pathognomonic dolor gradually manifests itself, and by the increasing burning or lancinating pain, chiefly referred to the sacral region, and always exacerbated at night, the patient is at length driven to seek relief, and that too frequently only when a cure is all but hopeless.

It therefore cannot, I think, be too often reiterated that it is possible to recognise the incipient stage of cervical cancer, should the patient fortunately come under examination before the advent of so-called pathognomonic symptoms or cachexia. I have elsewhere more fully referred to the methods by which this may be accomplished, but as their great importance and significance are not infrequently ignored they may be here

briefly recapitulated. In the first place, then, even at an early stage of the disease, as was long since pointed out by Montgomery, on careful examination the margin of the os uteri is found hard and often fissured. In the situation of the muciparous glands there are felt several small and distinctly defined projections, almost like grains of shot or gravel under the mucous membrane. Pressure on these with the point of the finger gives rise to pain or nausea. The circumference of the os uteri feels indurated or turgid, and is of a deep crimson colour, or, if eroded, presents some slightly projecting points, which bleed readily on touch. These nodules when excised will be found under microscopic examination to afford all the characteristics of malignancy, and hence the cautious employment of the curette so as to bring away a sufficient amount of the abnormal tissue for microscopic examination should be resorted to in every case of suspected or possible adenomatous or cancerous degenerations. I have to thank my colleague, Professor McWeeney, for having by the examination of such specimens afforded me the evidence of malignancy in several cases long before any characteristic hæmorrhage, discharge or pain was complained of, and so enabled the removal of the disease whilst it was still, as I believed, thus curable.

Amputation of Cancerous Cervix Uteri.—I have already stated that in the vast majority of the cases of uterine cancer that came under my observation the disease commenced in the vaginal portion of the cervix, and in only one-fifth of them did it apparently originate in the body or fundus uteri. Moreover, in that large proportion of such cases the cancer apparently remained, for a time at least, localised within the cervical zone, whence, as clinical experience demonstrates, it may in some cases be effectually removed by the timely amputation of the cervix above the area of malignant infection. Nor do the reasons, properly applied in other cancerous affections to the removal of a portion of the organ implicated, appear to me to justify the objections raised on such grounds to that procedure in the cases now referred to, inasmuch as the cervix is structurally distinct from, although anatomically continuous with, the body and fundus of the uterus. I shall not, however, dwell on this point, but will

merely mention some of the facts on which the views stated are based.

The Results of Early Removal of the Cancerous Cervix.—With regard to the immediate consequences of cervical removal for cancer I can speak with some certainty from my own experience. Whilst, as to the probability of the subsequent recurrence of the disease in the uterus or elsewhere, even at possible risk of being deemed presumptuous in formulating an opinion from a comparatively limited number of cases, such as those in which I have been able to follow up the after-history of the patients thus operated on, I may here venture to summarise the facts at my disposal, and to mention the inferences that appear to me thus justified. In the first place, I have so far seen no mortality directly consequent on the operation. Secondly, of thirty-one cases in which I amputated the cervix for cancer or supposed cancer and was able to trace the result, in one instance the disease returned in the uterus within four months, in five cases it returned there or elsewhere within a year, in two cases within two years, in one within three years, and in one the patient came back nearly four years after discharge from hospital with labial cancer. On the other hand, in ten cases there was no return of cancer within a period of four years, and I might mention that some patients I thus operated on more than ten years ago have since remained free from it. In five other cases the information procurable was limited to two years, and in six cases to one year after the operation, and indicated no recurrence of the disease within those periods.

These results may, I venture to think, be favourably contrasted with those obtained by the probably bolder, but not necessarily more successful, surgeons, who unhesitatingly, and as a general rule, advocate and practise the complete removal of the uterus in every case of carcinoma localised in any part of that organ. For, as just shown, the immediate mortality of the amputation of the cancerous cervix in my cases was *nil*. Moreover, of the patients so operated on two-thirds were apparently free from cancer at the end of the subsequent periods referred to, and, as far as I know, have since continued so.

The Method of Amputating the Cancerous Cervix which

was followed in these instances is somewhat different from that more generally adopted, and may, perhaps, be here briefly referred to. With the exception of some instances, in which either supra-vaginal amputation or the vaginal flap operation with the knife was resorted to, in the great majority of cases I have relied on the infra-vaginal removal of the primarily affected neck of the womb either by the steel wire écraseur or by thermo-cautery. But, before this was done we invariably, in the former instance, transfixed the cervix above the supposed limits of disease by a steel wire ligature so as to prevent retraction of the divided part, and to allow of its being drawn down for that subsequent thorough and deep cauterisation with the actual cautery of the stump, which, when the thermo-cautery had not been used for its separation, is, I think, always essential, and frequently successful in preventing any recurrence of cancer in the dense fibrous cicatricial tissues thus left. It need hardly be added that the necessary measures were taken to secure patency of the uterine orifice in these cases.

Hysterectomy for Uterine Cancer.—Having elsewhere discussed in detail the published statistics of the various operations for removal of cancerous uteri by vaginal and abdominal methods, I shall not here occupy time with these points. I fully admit, and in my own practice have recognised the fact, that in some instances—viz., either when the disease has distinctly originated in the fundus or body of the uterus, or as a palliative measure, in some advanced cases of uterine cancer—hysterectomy may be an unavoidable necessity. Nor can anyone for a moment question that its immediate mortality, more especially by the vaginal procedure, has been now reduced to a very small proportion of the cases operated on. None the less do I still maintain that these successful immediate results in no way represent the ultimate or curative consequences so obtained, and that they afford no ground *per se* for the adoption of those operations, as a general rule of practice, in the cases under consideration.

We have superabundance of statistics to show in how many cases of uterine carcinoma hysterectomy has been performed, and in how large a proportion of cases the patients were discharged, apparently convalescent, from

hospital a few weeks afterwards. But before we can accept those statistics as any proof that the patients in question were cured by the operation, or even that they lived longer than would otherwise have been the case, the still imperfectly discharged onus of establishing these facts devolves on those who rely so exclusively on such procedures. The accuracy of the published statistics of the results obtained in some cases where patients were considered as cured a few months, or even a year or two, after hysterectomy is in no way affected by the refusal to accept the conclusions thus arrived at. For if in such statistics be included, as is probably the case, the majority of instances of uterine cancer—viz., those in which the disease is located in the cervix—there need be no marvel, in the present advanced stage of intra-peritoneal and pelvic surgery, at the results so obtained. And I still venture to think that no less beneficial consequences, to say the least of it, might probably, as already shown, have been obtained with lesser risk by the timely removal of the affected cervix. But if, on the other hand, they refer to cases in which the upper regions of the uterus were extensively implicated by cancer, I must only confess my incompetence to realise how structures of such intense vascularity and intimate connection with lymphatic plexuses emptying into the vessels of the broad ligaments, as the fundus and body of the uterus, can long be the seat of cancerous changes without almost inevitable extension of the disease beyond possibility of capture by the surgeon's knife. Hence, under such circumstances the ultimately successful or curative results of hysterectomy must, I fear, be regarded as largely fortuitous and exceptional, although in some cases the fatal issue may possibly be thus staved off for a time.

Any probable extension of life and immediate relief from suffering are obviously justification of whatever measures may afford the best chance of such results. On these grounds, therefore, and merely as a palliative, I have, in compliance with the urgent solicitation of the patient, resorted to hysterectomy in a few—viz., in only five—of the many cases of carcinoma affecting the upper portions of the uterus that I have seen within the long period referred to. There are, of course, no

definite conclusions to be drawn from so limited a number of instances, but I may, nevertheless, observe that in all but one of these exceptional cases of hysterectomy for cancer the patients recovered from the immediate effects of the operation, and were for a time relieved from their previous condition of misery. The ultimate results were, however, to a large extent in accordance with my anticipations. In one of these cases the disease returned in the bladder within five months, in another the patient died of disease of the spinal cord, which I believe to have been malignant, eighteen months later, and in only one was the patient alive, and apparently free from cancerous recurrence, at the end of two and a half years, when I last heard of her.

Palliative Treatment of Uterine Cancer.—In the majority of cases in which carcinoma uteri has not been recognised and removed before extension has occurred beyond its primary situation in the cervix, our chief function must, I fear, be the alleviation of suffering and the mitigation of symptoms. The methods by which those objects were attempted or accomplished in the inoperable cases of uterine cancer under my notice may be here recapitulated—firstly, with regard to the relief of pain and discharge, and secondly, with regard to the local applications by which an effort was made to arrest for a time the progress of the disease.

Relief of Pain in Inoperable Uterine Carcinomata.—Although I have found none of the newer hypnotics or analgesics, with the exception, perhaps, of orthoform, so promptly effective and reliable in giving ease to the pain of uterine cancer as the older fashioned preparations of opium, such as Battley and the acetum opii, I would venture to deprecate their too frequent exhibition in such cases. In the advanced stage of uterine cancer, when from the extension of the disease, more especially to the bladder or ureters, sedatives are most likely to be required, opiates or morphin cannot be habitually efficiently used without such increase in their dosage as to derange the digestive functions, and the nervous system and thus probably accelerate the fatal issue. Moreover, under these circumstances I have noticed a special intolerance of the hypodermic use of morphin, and have seen what under ordinary conditions would have been a perfectly

safe hypodermic injection of this agent followed by "that sleep which knows no waking." Hence, in the cases referred to, we have in the hospital, as a rule, endeavoured and generally succeeded in affording sufficient sleep and mitigation of pain by ringing the changes in succession in the various old and new analgesics as each in turn lose their effect. Amongst those thus employed were the bromides of potassium and sodium, trional, chloral, sulphonal, belladonna, conium, and lastly, the newest and probably one of the most generally useful pain allayers in uterine cancer cases—viz., orthoform.

Deodorisation of Cancerous Discharge.—As already observed, many patients in an advanced stage of uterine cancer suffer still more from the fœtor of the vaginal discharge than from the accompanying pain or hæmorrhage. The mitigation of this source of discomfort is therefore a matter of great practical importance in such cases. The mere frequent syringing with warm water, occasionally relied on, is, *per se*, almost useless for this purpose, whilst some of the deodorants, such as iodoform and ichthyol suppositories, sometimes prescribed, are only less offensive in their own odour than the discharge, the fœtidity of which they are designed to mask. Nor is the desired object sufficiently effected by the ordinary antiseptic solutions of izal, carbolic or boric acid, &c. Of such agents one of the most effective that I have employed is peroxide of hydrogen—which, even for some hours after its use, leaves the patient free from this horrible addition to her miseries. But, as the cost of this agent is a bar to its use by the poorer class of patients, in the majority of such cases, we may be enabled to overcome the characteristic smell of cancerous vaginal discharges almost as effectively by the use of one or other of the cheaper as well as the best of all deodorants—viz., firstly, chlorate of sodium in the proportion of a couple of drachms to a quart of hot water; or secondly, by a one per cent. solution of formalin; and thirdly, by the use of turpentine. The latter can be thus employed by putting half an ounce of pure turpentine with a spoonful of magnesia into a quart of boiling water, and then pumping the mixture with the syringe from one vessel into another until the temperature

will be reduced to blood heat, when the turpentine at the same time will be thus mechanically sub-divided, and will remain diffused through the fluid for a sufficient time to allow the vaginal passage to be washed out before the oil comes together again. No deodorant or styptic application in general use appears to afford more distinct respite from the fœtor as well as hæmorrhage of cancerous uterine discharge than this.

In some cases, however, similar effects can advantageously be produced by applying a small tampon saturated in a mixture of pure terebene and glycerine to the cancerous surface, where it may be left *in situ* for several hours.

I may now refer to some of the other local applications to which I have given a trial in these cases.

Methylene Blue.—More than five years ago I called attention to the value of this agent as a local analgesic in pruritus and other gynæcological cases, and since then I have frequently employed it in this way and by internal administration to relieve the pain of uterine cancer. In cases of inoperable cervical carcinoma a pledget of sterilised gauze saturated in a five per cent. solution of methylene blue will commonly not only allay pain but also cleanse and temporarily improve the condition of the part, whilst the injection of a similar solution by the needle into the substance of a medullary growth will frequently for a time cause some apparent diminution of its size and a decided abatement in the amount and fœtor of the discharge.

Local Injections of Absolute Alcohol in the manner recommended by Schultz were employed in some of my inoperable cases of cervical cancer. In one of these the first injection of alcohol was followed by such intense local pain and constitutional disturbance as to prevent its repetition. In two other cases similar but deeper parenchymatous injections repeated at intervals of two or three days were attended with some diminution in the amount and fœtor of the discharge and apparent shrinkage in the diseased structure. But in neither of these cases did the patient remain sufficiently long under observation to warrant any conclusion as to the probable duration of these effects.

Greater Celandine or Swallow-wort, in the form of a liquid extract, which a few years ago was reintroduced as a remedy for

cancer was, on the suggestion of my colleague Dr. Redmond, who was then trying it in other cancerous affections, employed in several of the cases of uterine disease of this class in my wards, and with results somewhat similar to those he obtained. In three cases in which the celandine was administered internally and applied locally, the condition of the cancerous ulceration was rapidly and distinctly improved for a time; in two no change was produced, and in none of them was any permanent or curative effect produced during the period the patients were under my observation.

Serum-therapy.—As the treatment of uterine carcinoma by the injection of the Streptococcus erysipelatis and other serums has been carefully investigated and unfavourably reported on by a Committee of the New York Surgical Society, and hence as there appeared to be no justification for the further trial of these agents, they were not employed in any of the cases here referred to.

Potential Cautery or Escharotic Treatment.—In cases where the disease is beyond possibility of removal by operation, or where it is apparently returning in the cicatrix after operation, or where by the destruction for a time at least of the cancerous tissues we may hope to afford relief from pain and discharge or to arrest hæmorrhage, and so prolong life and render death more easy, escharotics may in some instances be usefully employed. Of those which I have myself tried in such cases, including nitric and chromic acids, acid nitrate of mercury, ethylate of sodium, and chloride of zinc, I have found the last unquestionably the most effective, safest, and least painful, and have long since discarded other agents of this kind in any case of uterine cancer.

In some cases of extensive cancerous disease of the fundus or body of the uterus, and even when the cavity was filled and enlarged by the malignant outgrowth, and the patient's strength greatly exhausted by the accompanying hæmorrhagic and fœtid discharge and pain, I have succeeded apparently in prolonging life for a considerable time, and yet more distinctly in relieving the symptoms by thoroughly curetting away as far as possible the cancerous structures and freely cauterising the thus exposed uterine wall with chloride of zinc.

Actual Cautery.—In two instances of advanced cervical cancer with glandular complications and considerable discharge and hæmorrhage, I destroyed the diseased surface by deep cauterisation with the actual cautery as a local palliative. In the first instance the eschar sloughed out, leaving a cleaner and comparatively healthy looking small ulcerated surface. In this case the cauterisation afforded marked relief from pain, hæmorrhage, and discharge as long as the patient remained under observation. In the second case no benefit was produced.

Electrolysis in Uterine Cancer.—As alternatives to the older escharotic methods the galvano-cautery or the negative electric current were also tried for a time in cases of carcinoma in my wards, on the strength of the success claimed for them by Drs. Apostoli of Paris, Byrne of Brooklyn, and Parsons of the Chelsea Hospital. The effects, according to the latter authority, produced by the passage of the current directly through the cancerous structure, consist of a cessation of growth, gradual subsidence of pain, shrinking and hardening of the tumour, followed by improved nutrition and improvement of constitutional condition. In the few cases in which I endeavoured to follow the directions published for the procedure some benefit was for a time evinced. But, possibly, from inexperience in the methods or insufficiency of perseverance, I was not myself enabled to secure the more marked and permanently good results hoped for from this practice in any of my own cases.

In conclusion, I have only to add that my clinical experience, extending over more than a quarter of a century in three hospitals, of the various measures there tried in the treatment of uterine cancer may be briefly summed up in the statement, that up to the present I know of but one generally feasible means of possibly curing this disease, and that is by its early recognition, and its timely and complete removal in the way which has been described in this communication.

ART. XX.—*On Room Disinfection, with special reference to Formalin Vapour as a Disinfectant.*[a] By T. PERCY KIRKPATRICK, M.D. Univ. Dubl.

IN considering the question of the prevention of tuberculosis, my attention was strongly arrested by the need there was for an efficient yet easily applicable mode of room disinfection. What appeared to me to be most desirable in such a method was simplicity, for there are many ways of disinfecting rooms, clothes, furniture, &c., which are of undoubted efficiency, but they are all of a more or less costly nature and involve considerable trouble in their employment. In the case of tuberculosis, however, what was wanted was a simple process, for the long period over which a case of the disease extends renders it absolutely necessary that any means used for disinfection should be often repeated if it is to be of any service. In cases of acute infectious diseases, such as fevers, &c., one application of the process may be enough, but in tuberculosis something more is wanted.

In view of this, if we examine the methods commonly used, we shall, I think, find that there is much to be desired. We may divide these methods into two great classes :— (*a*) Disinfection by means of germicidal gases, or "gaseous disinfection," and (*b*) disinfection by means of washing or spraying with germicidal solutions. Of course, we have combined with both these groups the various processes of mechanical cleaning.

The former class would at first sight appear by far the most suitable. It would be merely necessary to generate the gas and leave it to permeate all parts of the room without further trouble on our part; whereas in the employment of solution every part of the room, as well as every article in it, must be carefully gone over by the operator. Objects which would be injured by water must be removed to be treated by heat or some other method. The advantages of gaseous disinfection, then, are so obvious that we need not wonder at the numerous

[a] Read before the Section of State Medicine of the Royal Academy of Medicine in Ireland, on Friday, April 28, 1899.

efforts which have been made to introduce some efficient gas. In the efficiency though lay the difficulty, for a gain in simplicity will not be an advantage if it is made at the sacrifice of efficiency.

An examination of those methods of gaseous disinfection which have hitherto been employed will not show any very remarkable qualities in either of these respects, and in consequence of this their use had well-nigh been altogether abandoned in favour of the other methods.

The three gases which till recently had been most used were sulphurous acid, chlorine, and bromine. With the general public the first has long been the most popular; indeed, we hear of it in Homeric times when Odysseus used it to purify his house after the slaughter of the wooers. Its popularity depends mainly on its powerful smell; for, as regards efficiency, we must rank it lowest of the three. I wish briefly to refer you to some experiments by well-known investigators to show the conclusion which had been arrived at as to the possibility of gaseous disinfection.

First, as regards sulphurous acid, this was tried experimentally by Koch, and his results are very instructive.

The gas was generated by burning sulphur in an iron vessel in the room. He obtained a percentage vol. of the gas, at one hour after setting fire to the sulphur, of 2·89.

After 24 hours this had fallen to 0·02

After 48 hours „ „ 0·01

One pound of sulphur to every 1,000 cubic feet, giving 1·1 vol. per cent. of the gas.

Micrococcus prodigiosus and the micrococcus of blue pus after 48 hours' exposure in this atmosphere were found unaffected.

In a further experiment, the percentage vol. of the gas 1 hour after starting was 3·12, and 24 hours after starting it had fallen to 0·015.

From these and other experiments Koch concluded that for the purposes of practical disinfection sulphurous acid is useless. The gas disappears so rapidly that, even when the initial proportion is large, it fails to kill spores freely exposed after being thoroughly moistened. Where it is

less in amount, prolonged exposure had no effect on micrococci.

Besides this objection on the score of inefficiency, the gas is objectionable. The smell of the sulphur persists in the room for a long time even with free ventilation, and is very unpleasant. All metal and gilt ornaments, pictures, &c., must be removed or protected from the gas or they will be tarnished. It is evident, then, that little can be adduced in favour of sulphurous acid as a gaseous disinfectant, and we see that it is almost wholly discarded now by the profession.

Let us next examine chlorine. Drs. Fisher and Proskauer have experimented with it and bromine. I quote from their paper on the subject.

By preliminary experiments in an artificial chamber they arrived at conclusions as to the percentage vol. of the gas which it would be requisite to use for disinfection. The gas was generated by the action of hydrochloric acid on chloride of lime, and it was calculated that about 15½lbs. of chloride of lime was requisite for every 1,000 cubic feet to get the necessary percentage of gas.

It was further found necessary to saturate the air with moisture either by evaporating water in the room or by spraying. It was found also that for practical purposes the gas should be generated from several vessels, and that these should be placed near the ceiling of the room on account of the high sp. gr. of the gas.

Twenty-seven specimens of bacteria were exposed freely under these conditions for 24 hours, and out of this number 22 were killed.

14 out of 15 specimens of earth spores.
2 out of 4 anthrax spores.
2 out of 4 Micrococcus prodigiosus.
4 out of 4 Aspergillus niger.

The results were not so good when the specimens were sheltered, all surviving except one sample of Aspergillus niger. Articles of clothing, &c., which were exposed were found after the experiment to be discoloured, and to be so injured in texture as to be readily torn. We see, then, that though with chlorine most of the bacteria freely exposed

were killed, penetration was bad. The conclusion arrived at may be stated in the experimenter's own words. "Complete disinfection of rooms appears, therefore, to be unattainable by means of chlorine. . . . . Nevertheless, chlorine may be of great value as a disinfectant in many cases, since it is, at least, capable of destroying all organisms, even the most resistent, which lie on the surface. Hangings, &c., may be removed (for disinfection by steam) and the surfaces either washed with a solution of mercuric chloride, or scraped. A preliminary partial disinfection by means of chlorine would greatly lessen the danger of those employed in a subsequent and more thorough disinfection. In all cases where a gaseous disinfectant is needed, chlorine is the best at our command, being superior to either sulphurous acid or bromine." All hangings, carpets, clothing, &c., must be removed, as chlorine seriously injures them, and metallic surfaces must be protected by varnish or vaseline. The cost of disinfection amounts to about 4d. per 1,000 cubic feet.

When we consider these results it will be obvious to everyone that for the purpose we had in view chlorine was quite unsuitable. It has little or no advantage over the more certain methods in the ease of application, and the difficulties involved in its use quite prevent it ever coming into general employment.

Disinfection by bromine gas is almost entirely prohibited by the cost—1/- per 1,000 cubic feet—and it does not appear to have any advantages over chlorine, except, perhaps, a more uniform diffusion, however, which is compensated for by greater sluggishness.

We are consequently forced to the conclusion that none of the conditions which I postulated as essential for a gaseous disinfectant are fulfilled by those commonly in use. They are not in any case absolutely efficient, nor are they easily used. Of the three, the sulphurous acid is the most convenient, but, as Koch says, it is practically useless. To disinfect with chlorine is perhaps as troublesome as with some of the germicidal solutions, while it is not as efficient. We cannot wonder then that gaseous disinfection fell into disrepute, and was generally neglected. Though we

cannot deny the efficiency of the other methods of disinfection, still it is obvious that for the purpose I had in view they were useless. The trouble and expense involved in their frequent use effectually prevents them from helping us here. What was wanted was a simple, cheap means of effecting complete surface disinfection of an ordinary dwelling-room and its contents without damage to any of them. Formaldehyde appeared to be the very thing required, and it was with the object of testing the claims put forward on its behalf that these experiments were undertaken. The gas is easily generated by means of a lamp or "altformant," sold at a moderate price by Messrs. Zimmermann. This apparatus is perfectly simple in structure. It consists of a hollow tin cylinder, at one end of which is a reservoir with lamp for burning the methylated spirits, while into the other end fits a metal chamber for holding the paraform tabloids. The action of the products of combustion of the spirit on the paraform is to convert it into formaldehyde in a gaseous state. This gas CH_2O has, it is said, a sp. gr. almost equal to that of the air, so that it diffuses rapidly to all parts of the room. While possessing a powerful germicidal action, the gas has no injurious effects whatever on fabrics, metals, &c., consequently it is not necessary to remove anything from the room in which it is used. When undiluted it has a powerful and penetrating odour, and is irritating to the mucous membrane of the nose and conjunctiva, but it does not appear to be poisonous to animals.

It is claimed for the gas that it is a deodorant and germicide of great power, and that it has considerable power of penetration into clothes, &c. It is very easy to manipulate, safe and not costly, so that if it can be shown to be efficient it would appear to be an ideal disinfectant. The penetrating smell, which, I may remark, rapidly disappears on dilution with air, further recommends it to the public; for it is, as I said before, by means of their noses that the public generally estimate the value of a disinfectant. A vast quantity of literature has already accumulated about the uses of formaldehyde, and many experiments have been undertaken to test its efficiency as

a germicide. Perhaps I may be permitted to bring a few of the more recent of these to your notice before passing on to those made by Dr. Littledale and myself. Drs. Fairbanks and Grawitz, of the City Hospital, Charlottenburg, Berlin, used for their experiments an ordinary sleeping-room of 3,213 cubic feet capacity, or 93·6 cubic metres, and diffused 1 gramme of formaldehyde to every cubic metre, or 1 tabloid to every 35 cubic feet. The room was closed for 30 hours, and various cultures were exposed.

(*a*) Anthrax exposed freely at various heights. All remained sterile, and were injected into mice without any ill results.

(*b*) Anthrax exposed between bits of cloth on the floor showed no sign of growth.

(*c*) Anthrax exposed as in (*b*), but placed between two mattresses, showed abundant growth after 20 hours.

Bacilli of diphtheria and typhoid fever and staphylococci freely exposed gave no growth.

The surface of the dust appeared sterile, but showed growth of bacteria in its deeper parts. Tubercular sputum dried in vacuo and exposed in a glass vessel for 30 hours was then rubbed up with sterilised water and injected into guinea-pigs without any ill result. Control experiments gave positive results with guinea-pigs.

The conclusions which we appear to be warranted to draw from these experiments and from numerous others by different observers appear to be that, while the disinfectant action on the surface of objects is very efficient, its penetrative power is not very marked—in other words, formaldehyde has as great disinfectant powers as the best of the gases hitherto used, if not greater, while it is free from many of the objections to which the others were open.

If we compare these results with those to be detailed by Dr. Littledale, we shall, I think, find the same general result, though in his case the quantity of gas was not so great (about $1\frac{1}{2}$ tabloids to every 100 cubic feet), nor was the exposure so long (9 hours). The experiments with the pus-smeared gauze and with the sputum on the cover

glass are very interesting, showing as they do the powerful germicidal action of the gas on the surface of such objects.

It may be objected that the formaldehyde was present in the exposed objects when they were inoculated on the culture media, and that in consequence, though growth was prevented, we are not warranted in assuming that the micro-organisms were dead. The answer to this is, that when the inoculation was made on fluid media, where the gas could easily disperse, the results were the same. I believe this objection may have some force when solid media are used, and may perhaps explain the various results obtained by different experimenters.

We are, I think, justified in concluding that in formaldehyde, generated as I have described, we have a most useful adjunct to our disinfectants. In efficiency it may not perhaps reach our ideal, still it possesses more of those characteristics which go to constitute that ideal than any other of the gases; and while this is so, it is equal, if not superior, in efficiency to any of them. As regards ease of manipulation, it far surpasses them all, while its safety is all that could be desired.

I firmly believe that great benefit will be derived, both by patients and attendants, from its regular use in the homes of the tubercular. We must gladly welcome every additional means to combat the spread of this dreadful disease, since those at our disposal are so inadequate, and especially will we welcome one which appeals to our reason as logical, and the claims of which have answered the test of experiment.

ART. XXI.—*Experiments on Formalin Vapour as a Disinfectant.*[a] By H. E. LITTLEDALE, M.B.; Assistant in the Pathological Laboratory, Trinity College, Dublin.

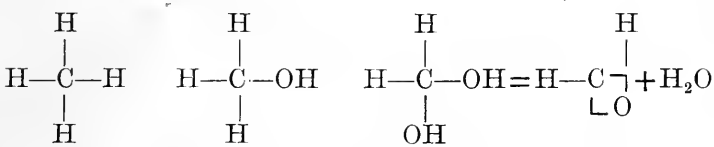
BY the kind permission of the visiting staff of Sir Patrick Dun's Hospital we were able to carry out our investigations,

[a] Read before the Section of State Medicine of the Royal Academy of Medicine in Ireland, on Friday, April 28, 1899.

and Dr. Haughton kindly placed at our disposal his X-ray developing room. This room is a small one of about twenty-eight cubic metres content, with no opening into it except the door, which fits very tightly, and a ventilator over it, which was hermetically closed before the experiments. Opposite the door there is a table, on which the altformant lamp was placed, and ten grammes of formalin tabloids were vaporised by heat. The door was closed tightly, and all crevices plugged with tow; and at the end of nine hours the door was opened, and the various test objects were placed in nutrient media.

As to the nature of formalin, it is a 40 per cent. solution in water of formic aldehyde.

Aldehydes are hydroxyl substitution products of the hydrogen in hydrocarbides, and formaldehyde is the simplest of the series.



Methane. Methyl-alcohol. Formaldehyde and Water.

In order to test whether the formalin vapour, generated as described, was really an efficient disinfectant and capable of destroying bacteria when exposed to it, experiments were undertaken in such a way as to copy the conditions usually present when one has to disinfect a room after, *e.g.*, diphtheria or scarlatina.

Several species of bacteria grown in different media were exposed in different ways, to be described later on. The following is a list of these bacteria:—

(1.) An emulsion of the common yellow coccus of the air made in boiled water, the coccus having been grown on gelatine.

(2.) A similar watery emulsion of a mixture of *Bacterium coli commune* and *Staphylococcus pyogenes albus* which had been grown together on an agar tube.

(3.) A pure culture of the typhoid bacillus growing actively in bouillon.

Besides these known bacteria, objects containing a mixture of unknown bacteria were also used. These were:—

(1.) Sputum from a case of cirrhosis of the lung, dried on cover glasses in an oven at 37° C.

(2.) Fresh gonorrhœal pus, smeared well into small pieces of plain unsterilised gauze.

(3.) A gelatine plate, on which were growing numerous bacteria out of putrid urine, some of which, however, had been specially marked, and proved to be a Micrococcus ureæ non-liquefaciens.

(4.) Dust from the wall of the room where the experiments were going to be made.

To expose the bacteria in the watery emulsion form would have been useless as a practical test for a gaseous disinfectant, so they had to be brought into a dry state in order that the vapour might be able to act on them.

In order to do this, clean silk threads, which were not sterilised, were cut up into small pieces about half an inch long and laid in watch glasses that were also just ordinarily cleaned with boiled water and dried over a flame. Several drops from each of the liquid test objects were then poured over them, and they were left for about ten minutes to soak well. They were then taken and wrapped in pieces of clean, but unsterilised, filter paper, so that one had several little loosely-folded packets of threads, soaked in typhoid bouillon, in an emulsion of *Bacterium coli commune* with *Staphylococcus pyogenes albus*, and in an emulsion of the air yellow coccus, and, added to these, there were also the cover glasses of dried sputum and the pus on gauze, which, however, was not quite dry when the experiments were started.

Tubes of nutrient media, inoculated just before the experiment, were also used as tests.

Three test tubes containing a sloped agar surface, and two containing sloped blood serum, were prepared in the following way:—

One agar tube, 12·5 cm. long by 1·5 cm. wide, was smeared with a mixture of *Bacterium coli commune* and *Staphylococcus pyogenes albus*, taken from an agar tube, on which they were growing together, the length of the smeared surface being 6·5 cm. from the bottom of the tube. A second

agar tube, exactly the same measurements and smeared to the same extent with the same bacteria. Control tubes of both these were made.

One agar tube, 15·75 cm. long and 1·5 cm. wide, smeared with a pure culture of typhoid over an extent of surface 7 cm. from the bottom of the tube.

Two blood-serum tubes, 15·5 cm. long and 1·5 cm. wide, smeared with dust from the room over an extent of 4 cm. from bottom of the tube.

The tubes were all left open on a table in the room, on the same level as the gas generator and four feet away from it. One of the agar tubes containing coli and coccus was inverted, and one of the serum dust-smeared tubes kept as a control.

The three control tubes were put into the oven at 37° C., and kept there for twelve hours, after which time they all showed a most luxuriant growth the entire length of the smear.

When the experiment was over—that is, after the test objects had been nine hours exposed to the gas—the tubes were left till all smell of formalin vapour had left the room, then their wool plugs were reinserted, and they were placed in the oven at 37° C.

The blood-serum tube smeared with dust grew over the whole extent of the smeared surface, that is, 4 cm.; so the vapour was apparently unable to penetrate at all events any further than 11·5 cm. into the tube with efficient action.

The agar tube, 12·5 cm. by 1·5 cm., smeared with coli and coccus mixture over an extent of 6·5 cm., showed, after twelve hours at 37° C., a marked growth for 5·5 cm., which terminated in a sharply-defined horizontal line; so the vapour apparently penetrated the tube 7 cm. in an efficient manner, as even after five days at 37° C. no further growth above the transverse line took place.

The inverted tube grew over a distance of 4·5 cm., terminating abruptly just as the previous one; but the vapour appears in this case to have had a more penetrating action when ascending the tube than when descending it, as in this case it penetrated 1 cm. further into the tube. No further growth above the line after five days at 37° C.

The agar tube, 15·75 cm. by 1·5 cm., smeared for 7 cm.

with typhoid, grew, after fourteen hours at 37° C., over a distance of 3·75 cm., but no further even after weeks; so the vapour penetrated efficiently in this case to a depth of 12 cm.

The gelatine plate of urine bacteria was exposed open on a shelf several feet away from the gas generator, and about four feet above it. It was then covered and removed, and a colony of Micrococcus ureæ non-liquefaciens was picked off next day, and smeared on a fresh gelatine plate. No trace of growth took place even after four days on the fresh plate.

The threads, dried sputum, and pus-smeared gauze were exposed in the following ways:—

Several threads soaked in the air yellow coccus emulsion and let dry were exposed open in a watch glass on top of the experiment room door high above and far away from the generator. After the nine hours exposure they were embedded in gelatine that was semi-solid in a Petri dish, and kept at room temperature in September. These showed no trace of growth after five days.

Threads steeped in typhoid bouillon and let dry, were loosely wrapped in filter paper and placed in a piece of lint loosely folded and laid on a table near the generator. These embedded in gelatine showed no sign of growth after five days.

Threads steeped in *Bacterium coli* and *Staphylococcus pyogenes albus* were placed in filter paper loosely folded, and put into the middle of the Medical Directory. They also were sown on gelatine and showed no growth after two days, so one thread was transplanted on to agar and put in the oven at 37° C., where a most luxuriant growth developed after twelve hours, and at the end of three days colonies of both the coli and staphylococcus began to develop in the gelatine, and after five days the growth was profuse, the coccus beginning to liquefy the gelatine.

Threads of yellow air coccus emulsion dried and wrapped in blotting-paper, and then hung up in the pocket of a coat near the door. These were embedded in gelatine, but showed only a few minute colonies beginning to develop at the end of five days.

Threads in coli and *Staphylococcus pyogenes albus* emulsion

wrapped dry in filter paper and left on the table near the generator embedded in gelatine showed no trace of growth after five days.

Threads in yellow coccus emulsion dried and wrapped in filter paper, were placed in a watch glass on the floor in a corner of the room. Embedded in gelatine showed copious growth after two days.

Threads soaked in typhoid bouillon, dried and wrapped loosely in filter paper and then in lint, left on the table. These were dropped into bouillon and kept at 37° C., but showed no trace of growth after five days.

Threads in coli and staphylococcus emulsion dried, wrapped in filter paper, and hung up in the pocket of a coat near the door. Placed in bouillon, and kept at 37° C., showed a luxuriant growth and complete turbidity of the bouillon after twelve hours. Other similar threads to these exposed open in a watch glass on the table, then transferred to bouillon at 37° C., showed no trace of growth after five days.

Typhoid bouillon dry threads exposed open in a watch glass transferred to bouillon, showed no growth at 37° C. after five days.

Cover glass smeared with sputum and dried in the oven at 37° C., exposed open on a table, then transferred to bouillon and kept at 37° C., showed no trace of growth for three days, but on the fourth day the bouillon became quite turbid, due to the presence chiefly of a micrococcus. A control cover glass showed, under similar temperature conditions, a copious growth after twelve hours.

Two pieces of gauze smeared with gonorrhœal pus, one kept as a control, were partly dried, and the control put into bouillon, which became thickly turbid with numbers of different bacteria after twelve hours at 37° C. The other piece partly dry, exposed open on the table, and then transferred to bouillon at 37° C., showed no trace of growth for three days, but on the fourth day the surface of the bouillon was coated with a thin skin, which consisted of long threads of some form of leptothrix, but no other bacteria were present.

Control tests in bouillon of all the thread objects experimented with, showed copious growth after twelve hours at

37° C. These, then, are all the test objects exposed, and the results of the exposure. When I entered the experiment room nine hours from the time the experiments were started, the smell of formalin in the room was not very strong, and I was able to stay in the room, leaving the door open, though it was very unpleasant; the lamp was out, and there was nothing left of the tabloids in it. After leaving the door open for fifteen minutes, I started inoculating the tubes, &c., in the manner described after nearly all smell of formalin had gone.

Now let us consider what practical results we have got from these experiments. To begin with, they were undertaken under conditions far more favourable to the action of the disinfectant than one ever meets in actual practice. The room was a small one, with absolutely no opening into it except the door, which fits very tightly, and which was made more resistant to the escape of the gas by plugging the keyhole and underneath the door tightly with tow. Again the bacteria were exposed more or less openly, not buried in dust or lying in crevices, or in the middle of bundles of clothes, &c.

The experiments with the tubes which were inoculated and exposed openly are of especial interest, as they show that the vapour seems only capable of penetrating a tube efficiently for a certain distance, and what is more strange, its action ceases abruptly; but the actual distance of efficient penetration of the vapour for tubes of the same diameter is not constant, and it probably depends on the degree of resistance offered by the bacteria; still it is hard to explain in this way the sharply-defined termination of penetration as determined by the growth of the micro-organisms. Further, for the same bacteria the vapour seems to have a greater penetrative power where the tube is inverted than where it is erect. As regards the other test objects, it will be noticed that all the bacteria exposed open to the formalin vapour were killed with the exception of the threads of yellow air coccus, which were put under a table in the very corner of the room. These latter, however, were not quite openly exposed as they were wrapped loosely in filter paper. All the threads which were protected from the vapour, as, for instance, those hung up in a coat pocket, were very slightly or not at all

affected by the vapour, the only exception to this being the typhoid threads which were wrapped in paper and covered with several folds of lint.

The experiments with the sputum on cover glasses show that the vapour killed the surface bacteria, but when the surface layer had dissolved off in the bouillon, a condition easily recognised by the shaggy appearance of the smeared surface, then the bacteria in the deeper layers grew quite freely. In the case of the pus gauze, however, everything except a leptothrix was killed. All these experiments go to show that formalin, as a superficial disinfectant, is most useful, but its power of penetration is very slight. The results of a number of experimenters in Germany are quite in agreement with ours.

Dr. Gehrke, Assistant in Loeffler's Institute at Greifswald, published in the *Deut. med. Woch.*, No. 15, of last year, experiments which he conducted with Schering's lamp. He used in his experiments much stronger formalin, vaporising two grammes of trioxymethylene—that is, solid formalin pastilles—for every cubic metre. His results, however, were exactly like ours, as he found that protecting the test objects somewhat as we did was quite sufficient to prevent the gas getting at them; and, moreover, the inoculated tubes which he exposed gave exactly the same peculiar result.

Gemünd's experiments in Munich, with the same apparatus and under similar conditions, also gave results similar to Gehrke's.

Valagassa states that by using Trillat's apparatus, and developing the gas under 4 atmospheres' pressure, tubercle and diphtheria bacilli, in layers of sputum 2–3 mm. thick, are quite killed.

Petruschky exposed objects for one hour to vapour generated at 33 atmospheres, going down to two, in Trillat's apparatus, and got very good results, but failed to kill anthrax spores concealed in the toe of a boot even after leaving the gas, generated for one hour, to act for twenty-four hours after. His results with inoculated tubes were precisely the same as ours and Gehrke's.

Perhaps the most thorough experiments have been those of Elsner and Spiering, in Berlin Hygienic Institute. They

tested a number of generators, but obtained far the best results with an apparatus of Schlossman's. This apparatus works by generating formalin gas, mixed with steam and also glycerine, to prevent polymerisation.

Schönfeld, of Mannheim, corroborates the experiments of the two former with Schlossman's apparatus, and he makes the interesting remark that rabbits are not affected when left in a room where formalin is being generated from a Schering's lamp, while they are killed by the glycoformal steam of Schlossman's generator.

ART. XXII.—*A Case of Meningitis, probably Tubercular, followed by Recovery.* By TH. FINDLATER ZANGGER, M.D.; late Assistant Physician, University Hospital, Zurich.

DR. LINDSAY'S report on such a case causes me to give some details of a similar case which came under my observation in 1897, during a stay at Adelboden, in the Bernese Oberland. I regret my notes are not as detailed as I could wish.

A boy of four was seen by me on August 16th, 1897, in a semi-comatose condition, with all the symptoms of cerebro-spinal meningitis; fever had persisted for eight days. The boy had been getting rapidly worse from day to day, and was brought down in a basket from an Alpine hut, some 7,000 feet high, to the village (altitude 4,600 feet).

There was dilatation of the pupils, which hardly reacted to light, a temperature of 103·5°, a pulse of 130, absolute rigidity of the muscles of the back of the neck; from time to time spastic contractions of different groups of muscles of the arms and legs showed themselves, and the patient uttered from time to time the piercing shriek, which French authors have called "cri encéphalique." No paralysis; no signs pointing to typhoid fever or heart disease. The boy was an orphan, whose father had died some years before of a "rotten lung," to use the expression of the relatives. The case seemed hopeless, but I was agreeably surprised at being mistaken. The boy slowly recovered under symptomatic medicinal treatment and application of the ice-bag to the head. After three days he became gradually more conscious, did not cry except when moved, took iced milk,

had still rigidity of the muscles of the neck, a pain on movement on the 7th day, and a temperature of 103°, a pulse of 120. In a fortnight he could sit up, the fever had slowly subsided; he was still very weak. Relapse soon after, and when I left Adelboden on September 5th he was again better, with temperatures of only 102° to 101°, but persistent meningo-spinal symptoms.

Dr. Rohr saw the boy afterwards still, and reported him improving; further visits were declined by the relatives eight days afterwards as the boy was better.

My first visit to Abelboden in July, 1898, was to see my former patient. I found him running about and in fairly good health, afflicted only with a chronic flow from one ear for the last few months, for which treatment was not desired. This case seems instructive, as the diagnosis of meningitis is certain, and that of tubercular meningitis probable.

TYPHOID PARALYSIS.

Dr. N. Ritscher (*Bolnitschnaja Gaseta Botkina*, Nos. 45, 46, 1899), after reviewing the literature on the subject of paralysis occurring in the course of typhoid fever, described three cases that had come under his own observation. One of the cases presented a right-sided hemiplegia that developed on the thirty-fifth day of the disease; in the other, too, the left facial nerve was affected; in both instances the paralysis developed on the ninth day. As a result of his own observations and a careful research of the literature, he arrives at the following conclusions:—In typhoid fever paralysis of individual nerves, of entire nerve groups, paraplegias, hemiplegias, and finally general paralysis (ascending paralysis and progressive muscular atrophy), may occur. They develop either suddenly or gradually, almost always in the period of declining temperature. In the majority of cases the paralyses disappear after weeks or months, seldom remaining stationary (peripheral paralysis), or have a lethal termination (hemiplegia and ascending paralysis). They may be of functional or inflammatory origin (myositis, neuritis, myelitis, encephalitis), or result from emboli or thrombi. With the exception of emboli of the cerebral arteries, which originate from marantic cardiac thrombi, the paralyses owe their origin to the action of the typhoid toxins.—*Medical Record*, May 6th, 1899.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

General Physiology: an Outline of the Science of Life. By Max Verworn, M.D., Ph.D. Translated from the second German edition, and edited by Frederic S. Lee, Ph.D. With 285 illustrations. London: Macmillan & Co. 1899. Pp. 615.

At the head of the preface of this remarkable book the following statement stands as a sort of text:—"The elementary constituent of all living substance and the substratum of all elementary vital phenomena is the cell. Hence, if the task of physiology lies in the explanation of vital phenomena, it is evident that general physiology can be only cell-physiology." With the same sentence the work concludes, and this point of view is maintained throughout its course.

As to the class of readers for whom the volume is designed, the author says—"I wished to write something that would appeal first to my fellow-physiologists, and offer them, besides certain new facts and ideas, a summary of our scattered knowledge. But, at the same time, I wished the work to give to any interested scientific reader, whether a student of medicine, philosophy, botany or zoology, an outlook over the problems, facts, theories, and hypotheses of life; in other words, I wished to give him an introduction to general physiology, and thus afford him an idea of the important theoretical basis of his study."

In a short notice of such a work as this it will be impossible to do more than indicate the arrangement of the matter adopted by the writer; but we may say at once that the book, which has enjoyed a great success in Germany, and which has been translated, or is in course of translation, into several European languages, is one of the very highest interest and importance to everyone concerned in any way with biological study. There is no other work of just the same kind in

existence. The nearest to it is Claude Bernard's "Leçons sur les phénomènes de la vie communs aux animaux et aux végétaux," published in 1878–9. A comparison of the two works will, however, show how much our knowledge has advanced, and how widely our ideas have expanded in the course of the last twenty years.

The first chapter deals with the aims and methods of physiological research. The problem of physiology is defined as the investigation of life, and some consideration is given to the ideas which we are to attach to the ideas of life and of investigation or explanation. In a brief but very interesting sketch of the history of physiological research, the subject is divided into five periods.

1. The earliest times. Here we find some ideas which sound very modern, such as the notion of the derivation of man from animal-like ancestors originally inhabiting the water, as stated by Anaximander (620 B.C.), the struggle for existence, hinted at by Heraclitus (500 B.C.). "But the theory of Empedocles (504 B.C.) upon the origin of living things is the clearest and most surprising. According to him, plants appeared first, then the lower animals, and from them the higher animals, and finally men were developed by a process of perfection. The effective principle in this perfecting process he perceived in the fact that ill-adapted individuals are destroyed in the struggle for life, while those that are capable of living produce offspring." This is very like Darwinism 2500 years before Darwin.

2. The second period is that of Galen (131–200 A.D.), who first clearly perceived the nature and significance of physiology. He first recognised the importance of anatomical study and of experiments on living animals. The long continuance of his system shows its monumental character, although, no doubt, it was largely due to the darkness of the Middle Ages.

3. The period of Harvey (1578–1657).

4. The period of Haller (1708–1777).

5. The period of Johannes Müller (1810–1858). This great man, to whose memory Professor Verworn dedicates his book, is the father of the physiology of to-day. An appreciative sketch of his work is given, and it is pointed out how

much more comprehensive and philosophical the ideas of Müller are than are those of many of his followers at the present time. There are three great discoveries of the present century which have been already very fruitful, and from the further expansion of which physiology is justified in still expecting great results. These are the law of conservation of energy, the fact that organisms are composed of cells, and the theory of descent in the organic world.

The last section of the first chapter is on the method of physiological research. This includes a discussion of the relation of psychology to physiology, vitalism and cell-physiology.

The second chapter is on living substance, and deals with the individualisation of living substance, its morphological nature, its physical properties, its chemical properties, and the difference between living and lifeless substances. The views of Bütschli on the structure of protoplasm are accepted. "Protoplasm consists of a ground mass, in many cases completely homogeneous, in most cases very finely foam-like or honeycomb-like, in which lies embedded a greater or less quantity of very various solid elements or granules. In the foam-like protoplasm the granules always lie at the corners and angles, where the foam-vacuoles come together, never in the liquid of the bubbles themselves."

While the elements which compose living and lifeless matter are the same, in the former only few elements are found and these chiefly of the lowest atomic weights. "A special vital element does not exist, but the compounds in which these elements occur are characteristic of living substance, and in great part are absent from the inorganic world." They are firstly proteids, which are the most complex compounds known, and are never absent in living substance; further, carbohydrates, fats, and simpler compounds.

The following is the concluding paragraph of this chapter:—"We can thus summarise our considerations so far, and at the same time give a simple expression to the problem of all physiology. *The life process consists in the metabolism of proteids.* If this be true, all physiological research is an experiment in this field; it consists in following the metabolism of proteids into its details, and

recognising the various vital phenomena as an expression of this metabolism, which must result from it with the same inevitable necessity as the phenomena of inorganic nature result from the chemical and physical changes of inorganic bodies."

The third chapter deals with elementary vital phenomena. It is divided into three sections, the first dealing with the phenomena of metabolism, the second with the phenomena of form-change, including heredity, adaptation, cell division, and fertilisation, and the third with the phenomena of the transformation of energy.

The fourth chapter is on the general conditions of life. It also is subdivided into three sections. The first is on the present conditions, external and internal, of life upon the earth's surface. This highly suggestive section concludes as follows:—"A physical phenomenon takes place when, on the one hand, a material substratum is present in which it can take place, and, on the other, certain external conditions are fulfilled. The same holds good of vital phenomena. Vital phenomena appear with the same necessity that characterises the appearance of physical phenomena; when matter capable of life is present, and when the external general and special conditions of life are fulfilled. In other words, vital phenomena are an expression of the correlation of living substance and the surrounding medium."

This idea is still further developed in the second section on the origin of life upon the earth. The unsatisfactory nature of all previous theories on this subject is shown in a critical review, and the author draws thus his final conclusions:—"The fact stands out clearly and distinctly that life, from its beginning on, has been dependent upon the external conditions of the earth's surface. In a mathematical sense, life is a function of the earth's development. Living substance could not exist while the earth was a molten sphere without a solid, cool crust; it was obliged to appear with the same inevitable necessity as a chemical combination when the necessary conditions were given, and it was obliged to change its form and its composition in the same measure as the external conditions of life changed in the course of the earth's development. It is only a portion of the earth's

matter. The combination of this matter into living substance was as much the necessary product of the earth's development as was the origin of water. It was an inevitable result of the progressive cooling of the masses that formed the earth's crust. Likewise, the chemical, physical, and morphological characteristics of existing living substance are the necessary result of the influence of the external conditions of life upon the internal relations of past living substance. Internal and external vital conditions are inseparably correlated, and the expression of this correlation is life."

The third section is on the history of death. It is pointed out that there is no sharp limit separating life and death, but rather a gradual transition between them—that death undergoes development. An interesting description is given of the phenomena of necrobiosis, or "those processes that, beginning with an incurable lesion of the normal life, lead slowly or rapidly to unavoidable death." More particularly we would call attention to the description of what is termed granular disintegration. The view of Weismann on the physical immortality of unicellular organisms, which distinguishes them from mortal multicellular organisms, is critically considered and shown to be untenable. In conclusion it is shown that, while we are so ignorant of the composition of living substance, any attempt to produce life artificially must be futile. "For the present the task of physiology can consist only in the investigation of life. When physiology shall actually have accomplished this, it may think of testing the completeness and correctness of its achievement by the artificial inauguration of life."

Our limits forbid us to give any extracts from the fifth and sixth chapters, which deal respectively with stimuli and their action, and the mechanism of life. But we would, however, call attention to the interesting original observations of the author on the reaction of unicellular organisms to electrical and other stimuli detailed in the fifth chapter, and to the reasoning by which all the mechanics of the cell are derived from the metabolism of its substance, as given in the last chapter. It is shown in the successive chapters how the changes of form and energy are inseparably connected with

the change of substance, and while much remains unexplained, and many hypotheses are still necessary, "cell physiology, aided by the stern necessity of its development and its great working power, is beginning to give encouragement to the highest expectations."

This truly remarkable work cannot fail to mark an epoch in scientific literature. The views and arguments formulated in it, and the gradual development of the human mind which has made them possible, are among not the least of the achievements of the century now coming to a close. The book is one not only for the professional physiologist, but for everyone interested in science, or whose culture is sufficient to make him ask what he is, how he came here, or whither he is going. As such it must exercise a widely-spread and important influence. The translation must have been a work of great difficulty, but it has been pre-eminently successful. Professor Lee has given us an English version, which in every page is delightful and fascinating reading, and by this has ensured a greatly extended circle of readers and a more widely-spread influence for the book. The work is one which we cannot too highly recommend to the attention of every cultured man, and more particularly to those who, like our readers, spend their lives in the study of the phenomena of life and death.

Ovariotomy and Abdominal Surgery. By HARRISON CRIPPS, F.R.C.S.; Operator for Abdominal Sections to the Ward for Diseases of Women in St. Bartholomew's Hospital. London: J. & A. Churchill. 1898. 8vo. Pp. 624.

WE are at a total loss to understand how this work can be considered to have Mr. Harrison Cripps for its author, for by far the most valuable portions of it have come from the pens of others, while not a little of that which is accredited to Mr. Cripps is already familiar to those who are in possession of his former writings.

Mr. H. J. Waring is to be congratulated for his excellently illustrated and descriptive chapter on the Anatomy of the Abdomen; the Surgery of the Kidney is from the pen of Mr Bruce Clarke, and will repay our readers' close

perusal; while to Mr. C. B. Lockwood has been entrusted the chapter dealing with Radical Cure of Hernia, which is excellent so far as it goes.

It is, however, towards Mr. Cripps' portion of the work that we feel compelled to direct a certain amount of criticism, for we look upon it as altogether below the standard that should be expected from the writings of a gentleman holding his high hospital appointments. "I have" (he says) "been so frequently asked during the last few years by old pupils as to the best methods of fitting up an operating theatre in accordance with modern views, that I have described in some detail what has been done with this object in building and furnishing the special theatre attached to the woman's ward at St. Bartholomew's."

It will be perceived that the "best methods" of theatre construction had been asked for, and why then should a full description of "the Martha theatre" be afforded unless the author was fully satisfied with its perfection. The window of this theatre faces the south, a most objectionable aspect from an operator's standpoint, in consequence of the great variability of light and heat, and the dark contrast shadows which accompany sunbeams. A northern light is much to be preferred, as it is free from the above objections. There is a fire-place in Martha's theatre, and this of itself is enough to condemn the whole structure, as it offers an absolute barrier to a still atmosphere so greatly to be sought after when performing an aseptic operation. The wash-hand basins in Martha's theatre are highly praised, but apart altogether from the objection to having basins for preliminary disinfection in the theatre proper at all, the ones described are far from according with modern requirements; and the "tilt-up basin," which works on a pivot, and can be removed altogether for cleansing purposes, are, we think, vastly to be preferred; moreover, the water flowing into the basin is controlled by ordinary finger taps instead of the more cleanly feet levers.

Marine sponges are still employed by Mr. Cripps in abdominal sections, and as he does not use these a second

time we must arrive at the conclusion that he believes new sponges to be aseptic, nor does he know of any reliable method of rendering a septic sponge clean. Surely this childlike faith in the cleanliness of a newly purchased sponge is not in accordance with known facts.

If there is one position more important than another in which to place a patient while performing many abdominal operations, it is that of the Trendelenberg, and yet in Fig. 41 this is portrayed in a most incorrect manner, the pelvic elevation not being nearly great enough.

The author in his preface intimates that the object of this book is to record only such details as he himself has found of use. Apparently his experience has not been sufficient to furnish anything but an imperfect index of abdominal surgery. Let us take for example abdominal hysterectomy; extra-peritoneal and sub-peritoneal operations are described and contrasted as though they alone constituted the entire means at the disposal of modern surgery for the removal of myomata. The former is now an operation of the past, nor is there any one point that can be urged in its favour at present, while the latter, sub-peritoneal operation, when performed now, is carried out by methods far in advance of those advocated by the author, as, for instance, the "Kelly operation."

It is, however, probable that panhysterectomy is the operation that will find increasing favour in the future, if it is not already the procedure most constantly carried out nowadays, and yet it appears to have been only on one occasion performed by Mr. Cripps, and this quite unintentionally; neither does it receive any special description.

We now turn to the section dealing with inguinal colotomy, to see whether later experience has modified the author's views as to the advantages of this operation. We find him as great an enthusiast as ever, and so fond of the operation that he himself has performed it in over 300 cases. He refuses to discuss general statistics, and holds that the experience of individual operators of known skill will alone furnish a reliable index as to the usefulness of the operation.

We are told that his own special operation is one of

great nicety, and he has seen "the patient deprived of all the advantages of the operation by its ill performance from want of care or skill on the part of the operator."

To show us how likely his statistics are to be considered reliable, we read that he "had an opportunity of following many of these cases to the end," and "he has no hesitation in saying that the relief obtained, and the suffering avoided, are unmistakable." He again, however, warns his readers that colotomy is an operation of great delicacy, "requiring a good anatomical knowledge, with trained manipulative skill."

The writer has seen Mr. Cripps perform the operation of inguinal colotomy. The asepsis and *technique* may be matters as to which individual opinion may differ; but having watched the case in its subsequent stages, we think that it goes far to justify the opinion expressed by Dr. Gross—namely, "that the operations hould be discarded as amongst the obsolete devices of surgery." It is a circumstance also worthy of remark that Mr. Cripps in this instance made no effort to follow up the after-course of the case, which he could easily have done had he so desired, and this, to say the least of it, is surprising in view of the importance he attaches to personal statistics.

Annual and Analytical Cyclopædia of Practical Medicine. By Charles E. de M. Sajous, M.D., and One Hundred Associate Editors. Vol. III. Philadelphia, New York, Chicago: The F. A. Davis Publishing Company. 1899. 8vo. Pp. 600.

The third volume of this enterprising work follows the plan of its predecessors, and carries us in alphabetical order of subjects from "Dislocations" to "Myxœdema," inclusive.

Amongst the most striking articles in this volume are memoirs on "Dislocations and Fractures," by Professor Stimson and Dr. Keyes, junr., of New York; "Hip-joint Disease," by Dr. Reginald H. Sayre, of the same city; "Dysentery," by Dr. Flexner, of Baltimore; "Endometritis," by Professor Byford, of Chicago; "Infantile Myxœdema (Cretinism)," by Professor Osler and Dr. Norton, of Baltimore; "Exophthalmic Goitre," by Professor Putnam, of

Boston; "Goitre," by Professor Adami, of Montreal; "Gout," by Dr. Levison, of Copenhagen; "Eczema," by Professor Stelwagon, of Philadelphia; and an analytical study of "Hysteria" and "Hypnotism," by Professor Eskridge, of Denver.

Traumatic Separation of the Epiphyses. By JOHN POLAND, F.R.C.S. London: Smith, Elder & Co. 1898. 8vo. Pp. 926.

Skiagraphic Atlas showing the Development of the Bones of the Wrist and Hand, for the use of Students and others. By JOHN POLAND, F.R.C.S. London: Smith, Elder & Co. 1898. Royal 8vo.

MR. POLAND'S book on Traumatic Separation of the Epiphyses is the most complete work on this subject which has been given to the surgical profession. It is well that the author has issued separately the Skiagraphic Atlas of the Development of the Bones of the Wrist and Hand for the use of Students, for the size and cost of the complete work will, we fear, prevent its free use by the student; its size indeed renders it a book of reference rather than a text-book for the surgeon, who, however, will be grateful for its completeness and exhaustive character. We doubt if there be a single authentic case recorded that the author omits, and of the more important he quotes the descriptions verbatim. It is this exactness and completeness which has swollen the volume to 926 pages. The numerous (337) illustrations are, almost without exception, excellent. The skiagraphs, which represent the development of the wrist and hand, and are repeated in the Atlas, are very interesting, were it not that they bear too clear evidence of being touched up. There is a sharpness of the contours of the ossifying centres which suggests this. In our opinion a skiagraph should be published as it appears, without any interference. In the skiagraphs of the lesions of the superior epiphysis of the femur we see this—what we may call—tampering with the pictures most evident. Indeed, in one or two of these almost impossible forms are given to the bones; for example, in the representation of the neck of the femur of "a lad aged twenty-one." Pictures such as

these are very poor value and disfigure the book. The engravings inserted in the text are throughout excellent.

The fulness of the treatment of each epiphyseal lesion may be illustrated by the examination of a particular instance. To the separation of the upper epiphysis of the humerus one hundred and ninety-eight pages are devoted, with thirty-four illustrations; that this development of the subject of this chapter is not excessive is proved by the frequency of the errors of diagnosis which are so constantly occurring. On this point the author says:—

"In a large proportion of the cases described above a diagnosis of dislocation was at first made. Jetter says that out of seven cases operated on for deformity and loss of function of the arm after union had taken place, the displacement had, on five occasions, been diagnosed as a dislocation, and attempts at reduction had been made by the medical attendant.

"As late as June, 1893, we find (according to Lejars) a case being mistaken at one of the French hospitals, and vain attempts made at reduction by Kocher's method.

"Dr. J. F. Erdmann, of New York, mentions (*New York Medical Record*, October 26, 1895, p. 586) the case of a boy (M. M'G.) aged sixteen, who had fallen upon his shoulder six weeks before. He had been an inmate of one of the large New York hospitals, and was being treated for a subcoracoid dislocation of the humerus. There was no swelling, but marked flattening of the shoulder, broadening from before backwards, and $1\frac{1}{4}$ inch shortening. A slightly convex deformity, continuous with the shaft, with the bicipital groove well marked upon it, was found beneath the acromion, and downwards and in front, practically sub-coracoid, a round, fairly-moveable mass was felt. A diagnosis of epiphyseal separation was then made and operative treatment recommended, but refused. The author has seen the same mistakes made at the hospitals in London."

When, then, these mistakes are so common all the world over, and at the great seats of surgical teaching, we need not complain of our author devoting so many pages and illustrations in the hope of teaching the surgical profession the diagnosis of separation of the upper epiphysis of the humerus.

An examination of any other particular instance gives very much the same result. Lastly, each epiphyseal study is made very practical and complete by the discussion and illustration of the best mode of treatment. We can, therefore, commend this book with all confidence to the profession.

The Royal University of Ireland. The Calendar for the Year 1899. Dublin: Alex. Thom & Co. 1899. 8vo. Pp. 463.

The Calendar contains the usual information relative to the University and its several faculties. Changes in the courses and in the regulations for 1900 will be found at page 195, and should be consulted by all intending candidates. In October, 1900, one medical studentship, tenable for two consecutive years, of the annual value of £200, will be offered for competition among the Graduates in Medicine of the University. The subjects for examination will be Pathology and Bacteriology.

Burdett's Official Nursing Directory, 1899. Containing an outline of the Principal Laws affecting Nurses; particulars of Nurse Training Schools in the United Kingdom and Abroad; Nursing Institutions, &c., and a Directory of Nurses. Compiled and Edited, with the assistance of a small Committee of Medical Men and Matrons, by Sir Henry Burdett, K.C.B. London: The Scientific Press. 8vo. Pp. 672.

The nursing world is much indebted to Sir Henry Burdett, and we are glad to see an increase in the number of nurses' names in his Directory this year. We, however, look in vain for many Irish names well and honourably known to us, and we would urge upon all the duty of responding to the request of the editor, by filling in the forms which are sent to them. Under the heading of "Institutions Managed by a Committee," we regret to find that the notice of "The St. Patrick's Nurses' Home for supplying Trained Nurses to the Sick Poor," which appeared last year, has been obliterated. This needs some explanation. An institution established 23 years ago, and supporting a *permanent* staff of eight nurses, claims individual notice.

PART III.

SPECIAL REPORTS.

REPORT ON PUBLIC HEALTH.[a]

By Sir Charles A. Cameron, C.B., M.D.; D.P.H., Camb.; M. and Hon. F.R.C.P.I.; F.R.C.S.I.; F.I.C.; Ex-President, Hon. Dip. Public Health, and Professor of Hygiene and Chemistry, R.C.S.I.; Vice-President and Ex-President of the Royal Institute of Public Health, and of the Society of Public Analysts; Medical Officer of Health for Dublin; Hon. Member of the Hygienic Societies of France, Belgium, Paris, and Bordeaux, the Academy of Medicine, Sweden, and of the State Medical Society of California, &c.; Examiner in Sanitary Science, Royal University of Ireland; Member of the Army Sanitary Committee.

MIDDENS AND TYPHOID FEVER.

I have shown in previous reports that the evidence in favour of the water carriage system of filth removal, as against the storage in privies and middens plan, is overwhelming and conclusive. It is evident that the retention for months of human excreta in close contiguity of dwellings must necessarily have a pernicious effect on the atmosphere, and, in most cases, owing to pervious floors of privies and middens, on the soil too. Some years ago the tenement houses of Dublin were dependent solely on privies for their sanitary accommodation, but these objectionable Altars to Cloacina have nearly altogether been got rid of, and water closets substituted for them. In a large number of cases the substitution of water closets for privies was made by order of the police magistrates, at the suit of the Sanitary Authority, and it is remarkable that in no instance did the magistrate refuse to make such an order.

In Belfast the attempt of the Sanitary Authority to enforce the substitution of water closets for privies has not been successful, the magistrates generally refusing to make the required order.

[a] The author of this Report will be glad to receive any books, pamphlets, or papers relating to hygiene, dietetics, &c. They may be forwarded through the agencies of the Journal.

However, I am glad to be able to state that in a Belfast Improvement Bill now before Parliament, the Committee of the House, to whom the Bill was committed, approved a clause in it referring to privies. There are about 27,000 of these structures in Belfast, and nearly all of them are so situated that their contents have to be removed through the walls, and in many instances through rooms into the street. A clause in the Belfast Bill approved by the Committee of the Commons on the 12th of May last, enables the Sanitary Authority to enforce the substitution of water closets for privies. It also enables the sanitary authority to lend money to the owners of premises required to convert privies into water closets.

The Sanitary Authority of Eccles (near Manchester) last year obtained an order from a Court of Summary Jurisdiction for the substitution of water closets and of ash-bins or pits for midden privies. It was proved in evidence that the privies were infected with the dejections of enteric fever patients, and that nothing short of their abolition would ensure the destruction of the virus of the disease. An appeal to the Court of Quarter Sessions resulted in the confirmation of the decision of the lower court. The case was then carried to the Divisional Court of the Queen's Bench on a point of law. The court, consisting of the Lord Chief Justice and Mr. Justice Wills, reversed the finding of the two lower courts. The Town Clerk of Eccles, who was present at the final hearing of the case, stated to his Sanitary Committee that the following were the grounds of the judgment:—

"1. That the local authority having undertaken the duty of cleansing the privies in the borough, and not having so cleansed the privies in question as to render them free from infection by the typhoid bacilli which were proved to have existed therein, were responsible for the nuisance caused in that respect. (The owner having acquired the premises some time after the occurrence of the first case of typhoid fever, without notice of its existence, could not be held to be responsible.)

"2. That there being no evidence in the statement of the case by the Court of Quarter Sessions that the structures were different from those ordinarily in use and allowed by the Public Health Acts, structural alterations did not appear to the court to be necessary or proper to abate the nuisance.

"3. That the court was of opinion that no power was vested in the local authority to require the provision of water closets in substitution for the privy middens, even though they were defective

in structure, and in this case caused a nuisance to arise which might continue after the defects in the said structures had been remedied."

In Dublin it has been the practice to proceed in reference to privies as if they were ordinary nuisances. That water closets should replace them is merely a suggestion. Perhaps if the Eccles authorities had served a notice to abate the nuisance created by the pestiferous privies, and not served a notice requiring their replacement by water closets, the Queen's Bench might have taken a different view of the case. As the matter stands it is a very serious one. As the law is interpreted by the two eminent judges above-named, the Sanitary Authorities cannot compel the owners of privies to replace them by water closets ; nor can they do so at the expense of the Sanitary Authority except with the consent of the owner. Surely legislation on this matter is wanted. In the next General Sanitary Act it is to be hoped that power will be given to the Sanitary Authorities to prescribe the manner in which the filth of towns is to be disposed of.

THE STERILISATION OF WATER BY FILTRATION.

The "Transactions of the British Institute of Preventive Medicine" contains an interesting and elaborate paper by Mr. Joseph Hunt, B.Sc., on the above subject. The object of the research, which was conducted in the laboratory of the Institute, was to confirm or reverse the inferences derived from the results of Dr. Hans Schoefer's, of Vienna, and other investigator's experiments on the Berkefeld filter. For this filter has been claimed the power of intercepting pathogenic bacteria, especially typhoid bacilli, even when ordinary water bacteria pass through.

Mr. Hunt's experiments were made under the following heads:—

1. Does the Berkefeld filter when freshly sterilised give a filtrate which is *absolutely* sterile ?

2. Provided the filtrate is absolutely sterile on commencing to use a freshly sterilised filter, for how long does the filter continue to yield such a sterile filtrate ?

3. Does the Berkefeld filter show any differential action on pathogenic organisms as against the numerous harmless water bacteria which are normally present in drinking-water ?

4. Are the various methods at present employed in using the Berkefeld filter equally efficacious ; if not, which is the best method ?

5. Can any practical recommendations be made as to the best mode of using, and as to the requisite frequency of cleaning and

sterilising the filter in order that it shall be an efficient safeguard against water-borne disease ?

The experiments seem to have been conducted in a most careful and scientific manner, and the conclusions at which the author arrived are as follow :—

"1. A large number of experiments have shown that when the filters are efficiently sterilised and the filtered water protected from all sources of contamination, they give on the first day of using an absolutely sterile filtrate when used in an approved manner.

"This is the case with all the forms of filtering candles examined, not only with natural water but also with water artificially infected with enormous numbers of bacteria from cultures, and also with broth cultures teeming with bacilli.

"In each case not a single bacillus could be detected in the filtered water from the freshly sterilised filters when proper precautions were taken in using them.

"2. Although the filter gives an absolutely sterile filtrate when used as above stated, it is necessary if such a sterile filtrate is desired continuously to re-sterilise the filter daily; for bacteria begin to appear in the filtrate in very small numbers on the second or third day of using. These bacilli are mainly, if not entirely, water bacteria.

"For all domestic purposes, however, the experiments show that unless there are special circumstances which make such a precaution necessary, a thorough cleansing and sterilisation need only be performed twice or even once a week.

"3. A long series of experiments, in which London tap-water was strongly infected with *Bacillus coli communis*, showed that this organism could not be detected in the filtered water over a period of thirty-nine days.

"The method used in searching for this organism in the filtered water never failed to detect it in the unfiltered water or the bacterial coating on the outside of the filter. During this period *Bacillus coli communis* was so detected on the outside of the filtering candle on the thirty-ninth day.

"In these experiments water bacteria could, however, be detected in the filtered water on the fifth day of using the filter, although they were present in exceedingly small numbers.

"4. Exhaustive experiments showed that with London tap-water strongly infected artificially with the typhoid bacillus not a single typhoid bacillus was detected in the filtered water over a period of twenty-six days, though detected in the unfiltered water up to the sixth day.

"As is well known typhoid bacilli ultimately perish in water which contains the water bacteria which are always present in natural water, but during the whole time in which they were demonstrated to be alive in the unfiltered water, they were never once detected in the water which had passed through the filter.

"5. London tap-water strongly infected artificially with the cholera bacillus was found after filtration through the filter to be absolutely sterile and, therefore, entirely free from the cholera organism.

"6. A long series of experiments with the travellers' 'N' filter have shown conclusively that the method of using these filters with a combined suction and force-pump, causing as it does sudden and violent changes of pressure on the outside of the filter, is one which gives very bad results with filtering candles, which give an absolutely sterile filtrate when used by a better method.

"The principle of the 'R' filters intended for travellers, in which atmospheric pressure forces the filtered water into an exhausted receiver, is one which is free from the above objection, and when used by this method absolutely sterile filtrates are obtained even when identically the same filtering candles are used which previously passed large numbers of bacteria from the very first, when used by the 'N' or suction and force-pump method.

"7. Seeing that the bacteria which first appear in the filtered water are harmless water bacteria, and that such organisms as *Bacillus coli communis* and the typhoid bacillus do not accompany them, even when they are present in large numbers in the unfiltered water, it seems that in all cases in which a drinking water is not under the suspicion of being contaminated, a semi-weekly or weekly cleansing and sterilisation of the filtering candle is all that is necessary as a precautionary measure against water-borne disease.

"Nevertheless, in all cases of contaminated water supplies or supplies known to be infected with disease organisms, a daily sterilisation of the filtering candle is advisable, thus ensuring an absolutely sterile filtrate.

"As an additional precautionary measure in cases of epidemic outbreaks, all possibility of harm from any chance defect in the working of a filter should be guarded against by boiling even the filtered water when used for drinking purposes.

"Wherever a daily sterilisation of the filtering candle is resorted to, it is advisable to have two candles, one of which can be cleaned and sterilised whilst the other is in use, in order that a clean and sterile filtering candle shall be available for use immediately a used one is removed.

"The volume of water required should be procured slowly at a minimum pressure and stored in a suitable manner, by which means the filter works more efficiently, rather than by the use of high pressures and a more rapid flow for a shorter period."

THE RELATIVE IMPORTANCE OF FLIES AND WATER SUPPLY IN SPREADING DISEASE.

Dr. M. A. Veeder, of Lyons, New York, read a paper on the above subject at a meeting of the Buffalo Sanitary Club, held on the 14th December, 1898. He pointed out that in the late war in Cuba the attempt to prevent the spread of diseases of the intestinal class by the use of the purest water or water purified by boiling was a failure. Of course, the failure was not due to the use of pure water, but from the circumstance of concluding that if the water was pure no other source of disease need be looked for. The author shows that it was not until late in the summer that it was discovered how potent for mischief were the flies—that they were the vehicles of contagion. During the recent campaign in the Soudan the influence of flies in spreading disease was observed.

Dr. Veeder remarks :—

"It is not to be denied that there are dangers in connection with water supply, but they are not the only or the chief ones in the case of encampments. To illustrate, the writer has seen fæcal matter in shallow trenches open to the air, with the merest apology for disinfection and only lightly covered with earth at intervals of a day or two. In sultry weather this material, fresh from the bowel and in its most dangerous condition, was covered with myriads of flies, and at a short distance there was a tent, equally open to the air, for dining and cooking. To say that the flies were busy travelling back and forth between these two places is putting it mildly. I presume that in the very case just mentioned, a microscope would have revealed fly tracks of more or less fresh excrement across the meat and bread and sugar and in the milk pitcher and water jar; the excrement so deposited doubtless contained the colon bacillus and the typhoid bacillus, as well as other varieties less harmful. Even in a private house, not at all uncleanly, I have seen typhoid dejections emptied from a commode, and the latter thoughtlessly left standing without disinfection within a few feet of a pitcher of milk just left at the door, both the commode and the pitcher attracting the flies, which swarmed about and went from the one to the other. Is it strange that there were numerous cases of the disease in that house and others in the house next to it?

I have seen a shallow old-fashioned water closet fairly buzzing with flies on a hot day, and all around it open windows and doors leading into kitchens and pantries and dining-rooms. A single case of typhoid would start a severe local epidemic under such conditions. Indeed, such observations have been made only too frequently. In encampments it is the common thing.

"To clinch the argument, and apparently leave no loophole for escape, I have made cultures of bacteria from fly-tracks and from the excrement of flies, and there seems to be not the slightest difficulty in so doing. Indeed, the evidence of every sort is so clear that I have about reached the conclusion that the conveyance of infection in the manner indicated is the chief factor in decimating the army. Certainly, so far as is known to the writer, nothing adequate has been said about it in current discussions. On the contrary, there have been intimations of the existence of ideas that are positively dangerous, if what has been here said is true. There is no doubt that air and sunlight kill infection if given time, but their very access gives opportunity for the flies to do serious mischief as conveyors of fresh infection wherever they put their feet. In a very few minutes they may load themselves with the dejections from a typhoid or dysenteric patient, not as yet sick enough to be in the hospital or under observation, and carry the poison so taken up into the very midst of the food and water ready for use at the next meal. There is no long, roundabout process involved. It is very plain and direct, and yet when thousands of lives are at stake in this way the danger passes unnoticed, and the consequences are disastrous and seem mysterious until attention is directed to the point; then it becomes simple enough in all conscience.

"As regards the precautions to be taken, it is not necessary to enter into detail. Personally the writer has insisted that disinfecting solutions, preferably that of blue vitriol, be kept constantly in chamber utensils, so that all infection may be at once destroyed where there is typhoid or dysentery. The use of this same solution in water closets generally is found to be very satisfactory, destroying all smell and causing the flies to cease their visits. Other measures will readily suggest themselves. The important point is that the danger be fully and adequately recognised."

OYSTER FEVER.

The *Practitioner* for March, 1899, contains an interesting paper on "Oyster Fever," by John William Moore, M.D., President of the Royal College of Physicians of Ireland.

It is now a well-established fact that enteric fever is occasionally caused by the ingestion of raw oysters, taken from beds polluted with sewage. The first paper on this subject was contributed by me to the number of the *Dublin Journal of Medical Science* for September, 1890. It was not, however, until several years later that general attention was given to the possibility of the conveyance of the virus of enteric fever by oysters; and now quite an extensive literature exists on this subject.

Dr. Moore in his paper recognises not only the danger to which the consumers of raw oysters are exposed if the molluscs come from filthy foreshores containing typhoid bacilli, but believes that two diseases beside enteric fever may be caused by the ingestion of unwholesome oysters. They may cause, first, acute gastro-enteric catarrh, or, secondly, a specific continued fever, probably due to ptomaïn poisoning.

Dr. Moore says that within a few hours after unwholesome oysters are eaten symptoms resembling those of cholera nostras present themselves; there are vomiting, purging, nausea, and intense thirst. He is inclined to believe that in the first instance the irritation is localised in the stomach rather than in the brain—that is, that the poison acts locally on the mucous membrane of the digestive canal.

As regards the continued fever produced by unwholesome oysters, Dr. Moore agrees with Dr. Sidney Martin that cases of food-poisoning are caused by poisonous albumoses formed from proteids during their earlier states of putrefaction. Even when the food eaten is not putrid it may contain pathogenetic micro-organisms, which set up toxic action and produce disease characterised by a period of incubation. Such cases may terminate fatally.

Dr. Moore narrates the following cases of what he terms *ostreotoxismus* (ὄστρεον, an oyster):—

"On Monday, September 28th, 1896, I was called to see, in consultation, Miss M. G., aged 30. She was fast sinking with symptoms of heart-failure supervening on peritonitis. Her history was that one evening early in September her uncle and she had supped on oysters. Both were seized with cramps, vomiting, and diarrhœa during the ensuing night. Her uncle quickly recovered, but she was never well again, pyrexial symptoms developing in the course of a few days. Even from the outset she was extremely prostrate, and suffered from profound depression of spirits. So far as I could ascertain, the illness was by no means a

typical typhoid fever, and the lady's attendant physician was at a loss to give a name to it, so anomalous were its symptoms and course.

"On the 20th of the same month (September, 1896) I had been called to see a physician living some miles from Dublin, who had been ailing, off and on, for a fortnight or so previously. His symptoms were—loss of appetite, occasional nausea, and recurring diarrhœa. He had been more acutely ill since the 17th, three days before my visit. I found his liver and spleen enlarged, his pulse varied from 92, at 11 15 a.m., to 96, at 8 15 p.m., but his temperature rose only from 100·2° to 100·5° in the same interval. His tongue was thickly coated and his breath was exceedingly heavy and offensive. His story was that on Tuesday, September 1st, he had eaten a dozen of oysters, shortly after which he was attacked with vomiting and purging. This gentleman remained feverish for a week or ten days, and was as prostrate as if he had had influenza. His temperature chart was quite atypical. He was constipated, and the presence of some rose-spots with moderate splenic enlargement alone pointed to typhoid as the malady from which he suffered.

"I will now briefly describe a case in which all three phases of oyster-poisoning, as I have defined them above, presented themselves in succession.

"On Thursday, September 29th, 1898, Captain E., aged about 36, stationed with his regiment in a cavalry barracks in Dublin, dined at regimental mess, the occasion being a special guest night. One of the courses consisted of uncooked oysters, taken from a barrel which had been brought to Dublin from Essex. Before morning he and many other brother-officers and their guests were attacked with acute gastro-enteric disturbance. Most of the victims recovered after two or three days' *malaise*. Not so Captain E. and a few others. Captain E., for example, developed an anomalous continued fever, lasting for a week or ten days. On the eighth day from infection, that is, on Thursday, October 6th, I visited Captain E., with Dr. R. A. C. Burnes. The chief symptoms were—bad taste in the mouth, foul tongue, thirst, and sleeplessness. The bowels were confined, spleen slightly enlarged, no rose-spots. Captain E. quickly improved, and in a few days was up and out. But this satisfactory state of things was not to last. After some days of apyrexia, the patient began to ail again, and gradually passed into an attack of fully developed typhoid fever, which lasted nearly four weeks. I saw him, in consultation, on the 6th, 8th, 10th, 13th, and 31st days of this attack, which was certainly one

of typhoid fever of ordinary severity, but without complications. In this view Dr. Burnes and Major Goggin, R.A.M.C., who saw him subsequently, entirely concurred.

"It was stated to me that two of Captain E.'s brother-officers developed true typhoid fever, and that one of them was attended in London by Sir William Broadbent, Bart., F.R.S., during his illness, which was of a severe type.

"My reading, then, of Captain E.'s case is that the first attack of acute gastro-enteric irritation failed to eliminate two poisons with which the unsound oysters were charged; that one of these poisons produced an albumose, or ptomaïn-fever; and that, towards the end of the third week from infection, the more slowly acting poison of typhoid fever produced the characteristic features of that disease."

LITERARY NOTES.

Messrs. Cassell & Company, Limited, announce the early publication of a new text book on orthopædic surgery by Mr. Jackson Clarke. The work is to consist of about 500 demy octavo pages, and will be fully illustrated. Besides furnishing a text book on the subject, it has been the author's aim to embody the many advances, including the Röntgen-ray process, that have been recently made in the department of surgery with which it deals. A special feature of the book is to be an exposition of the pathology of deformities as a basis of orthopædic surgery.

BIRTH OF FŒTUS PER VIAS NATURALES IN A CASE OF EXTRA-UTERINE PREGNANCY.

Malinonski (*Medizinskoe Obotsrenyoe*, 1898; *Obstetrics*, April, 1899) reports this case as occurring in a woman, aged thirty-eight years, a multipara. During labour abdominal palpation revealed one tumour, corresponding to the uterus, with a second smaller one attached to the former and on the right side. Pains becoming inefficient, forceps were applied, but were unsuccessful. Craniotomy was then performed and the child was removed, but the smaller tumour did not disappear. The operator's hand was then introduced into the empty uterus, and thence to the right Fallopian tube, where the placenta was found and removed. The woman made a good recovery.—*New York Medical Journal*, May 6th, 1899.

PART IV.

MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence.

Hypnotism as a Therapeutic Agent[a]. By Lewis More O'Ferrall.

Hypnotism in one form or another has been known to mankind from the very earliest times, and we have accounts dating back to nearly 2,000 years B.C., stating how the priests and soothsayers of the ancients put the infirm into a kind of hypnotic sleep and effected strange cures by magnetic influences. The loadstone or natural magnet was also largely used by our forefathers as a therapeutic agent, and one of the oldest Greek authors, Ætius by name, tells us that if held in the hand it is an infallible cure for gout. Nevertheless animal magnetism, or hypnotism, to use a more modern term, began to be scientifically investigated only in the time of Dr. Anton Mesmer, a German physician, who lived during the 18th century, and he it was who first employed it in anything like a scientific manner in medicine.

Mesmer was born May 23rd, 1733, at Weil, a small village near the point where the Rhine leaves Lake Constance, and was educated and studied medicine in Vienna. Very shortly after being qualified he went to Switzerland, and there met a very curious case of hysteria in a young woman. After the trial of various remedies without success, it struck Mesmer that perhaps magnetism might prove of some service. He accordingly consulted with a certain Fr. Hell, then Astronomer Royal in Switzerland, and obtained from him the necessary magnets. (By this time 18th century artificial magnets had superseded the old mineral magnets, as being more powerful, more portable, and more convenient of application.) Armed with his magnets Mesmer again visited his hysterical patient, and applied them to her. The effect was magical. After a single application she was greatly improved, and after repeated applications she got practically quite well.

[a] Read before the Catholic University Medical School Medical and Scientific Society, on March 27, 1899.

Mesmer was delighted at his success, but his joy was soon turned to chagrin when Fr. Hell gave out that the cure was entirely due to his magnets, and in no wise due to Mesmer, and when people began consulting Fr. Hell instead of himself to get cured.

Mesmer was the more annoyed because he was quite convinced that the cure was not due to Fr. Hell's magnets, but to some magnetic influence in his own person, which influence was merely aided by the presence of strong magnets. He was the more inclined to hold this view from the fact that he believed that while bleeding a patient the blood-stream varied according as he, Mesmer, approached or withdrew from the patient, even when no magnets were present. He considered that this was due to the fact that he in his own person possessed a magnetic influence greater than most other men, and that this influence on the blood-stream of his patient caused it to flow more or less rapidly, according as he was near to or far from him.

Now it so happened that Mesmer about this time made the acquaintance of a certain Roman Catholic priest, Gassner by name, whom he found was able to work cures without any magnets. Fr. Gassner declared that all diseases were really possession by devils, and that one individual might be possessed by millions of these devils at the same time, and he put down his hypnotic cures to the effects of religious influences upon these devils. If Fr. Gassner had substituted the word "microbe" for "devil" he would not have fallen far short of our modern ideas of disease.

Mesmer, however, who was exceedingly jealous of Fr. Hell, on seeing the cures worked by Gassner, gave out openly that artificial magnets were in no wise needed to work his magnetic cures, and he declared that he himself could work without them by means of his own personal magnetic influence. He found he was successful, and so in the year 1778 he travelled to Paris to noise his discovery abroad.

He did not from this time quite discard the use of magnets, but he used them merely to impress his patients the more, and to heighten the effect of other devices. In fact Mesmer, who it must be remembered was a physician as well as a hypnotist, was cunning enough not to rely upon hypnotism or magnetism alone in effecting all his cures. We are told, indeed, one rather amusing story about him which will show us how far Mesmer really relied on magnetism in certain cases. He had been called in to see a patient suffering from pleurisy with the object of curing the patient by magnetism. Mesmer managed on some pretext to send away for a time the

other doctor who was in attendance on the case, and in his absence Dr. Mesmer bled and blistered the patient, and then ascribed the whole cure of the case to magnetism.

Dr. Haygarth, of Bath, about this time—*i.e.*, the year 1779—performed some very interesting experiments. He had some false magnets made out of wood and metal, exactly like the real magnets, and then without letting his patients into the secret he employed these false ones in the same manner as the real ones had been employed. He found that he produced exactly similar results in all his patients, which clearly proved that the effects produced were in no wise due to magnetic influence, but rather to the imagination of the person experimented on.

Mesmer at the same time in Paris, although professing to believe that he worked by means of a force emanating from himself, nevertheless worked powerfully upon the imagination of his subjects. They were all ushered into a dimly lighted apartment through which scented perfumes were wafted, and in which the strictest silence was observed. They were then fastened to one another by cords and chains, and Mesmer himself, dressed as a magician, walked amongst them with a huge magnet, touching first one and then another, until he produced the results he desired. Many of the women fainted, others were either rendered highly hysterical and cried out, or were convulsed. Men also were thrown into convulsions, so that the spectacle was a weird one, and one well calculated to work powerfully upon the imagination of each fresh patient.

Such goings on naturally created a great stir in Paris, and eventually the Government appointed a Commission to investigate the matter. This Commission, amongst the members of which were Baillie and Franklin,[b] reported *unfavourably* of Mesmer and his methods, and declared that both from a physical and a moral standpoint mesmerism should be condemned. They contended that only in desperate cases should it be allowed—in cases where "*il faut troubler tout pour ordonner tout de nouveau.*"

The very adverse report of the Commission caused Mesmer to leave Paris, and from this date onwards he practised his art but little, and only privately. He died in Switzerland the 5th of March, in the year 1815. He left, however, many disciples, who carried on his work, but who employed less violent and less harassing means to effect their results.

[b] Franklin headed the list of signatures, but he never attended any of Mesmer's meetings in Paris; he signed the report merely on the strength of what he heard from other Commissioners.

The next great step which was made in the advance of hypnotism was in the year 1841, when James Braid, a Manchester surgeon, took up the study of the subject. He began his investigation as a complete sceptic, but he soon found that by the employment of certain means he could produce a sort of sleep, and a peculiar condition of the nervous system, which he termed "neuro-hypnotism" (fr. *νεῦρον* = a nerve, and *ὕπνος* = sleep). The prefix *neuro* was soon dropped for brevity sake, and hence the term hypnotism came into use.

Surgeon Braid was aided in his researches by the well-known physiologist, Dr. William Carpenter, whose name is connected with the theory of the reflex action of the ganglia at the base of the brain, and with the connection between hypnotism and this so-called reflex action.

Surgeon Braid and his followers used hypnotism largely in surgery to produce anæsthesia, and Dr. James Esdaile, in 1845, in Calcutta, began also to use it extensively in his practice for the same purpose. He has left us a record of 250 operations painlessly performed by the aid of hypnotism.

The great Liston used Braid's method also in some of his operations, and the well-known surgeons Velpeau and Broca both employed hypnotism successfully in twenty-four operations. Dr. Liébault, of Nancy, in 1860, was the next to advance the claim of hypnotism in medicine, and then the great Prof. Bernheim took up the subject, and in 1886 published his valuable work on hypnotism.

In England, Charles Lloyd Tuckey and others now began to employ the Nancy—*i.e.*, Liébault's—methods in medicine, and in 1892 Dr. Tuckey read a paper on the value of hypnotism in chronic alcoholism before the British Medical Association at Birmingham. During still more recent years many of us have had opportunities, even in Ireland, of seeing hypnotism employed by some of our leading Dublin physicians, foremost amongst whom in this connection I might perhaps mention Sir Francis Cruise.

We have now seen in a general sort of way how hypnotism gradually came to be recognised in medicine as a therapeutic agent. It now remains for me to state briefly the modern methods of inducing it, to discuss the modern theories regarding it, and to enumerate the various therapeutic uses to which it is put in our own day. The modern methods of inducing hypnotism may be classed under five heads—

1. *Mesmer's Method.*—This consists in a combination of many methods. Thus, Mesmer acted powerfully upon the imagination as

well as upon the sense of sight and the sense of hearing of his patients. He also brought the use of passes and of fascination to his aid.[a]

2. *Braid's Method.*—This consisted in wearying out the sense of sight by making the patient stare fixedly at a bright object for some time. Braid occasionally used *suggestion* to help him.

3. *Luy's Method.*—This is carried out by means of "Luy's Rotating Mirror." It was noticed that birds were caught by bird-catchers in France by luring them with a rotating mirror, and hence Dr. Luy, thinking the same method might be employed to hypnotise human beings, tried it and was successful. Its chief advantage lies in the fact that it saves the operator much trouble. The mirror is turned by clockwork.

4. *Charcot's Method.*—This consists in tiring out the sense of hearing by monotonous sounds. Charcot himself used a large tuning-fork for the purpose.

5. *The Nancy (or Liébault's) Method.*—This is the method most commonly adopted nowadays, and is carried out as follows:—Place the patient in a comfortable position with his back to the light, and then press gently upon his eyeballs, with his eyes closed, having prepared his mind as far as possible for hypnosis. Then let him hold a small disc slightly above the level of his eyes, and at a distance of six or eight inches from them, so that there is slight strain produced upon the capsule of Tenon[b] and on the muscles of the eye, and at the same time use suitable suggestions. These simple means, after a little time, usually suffice. But they may not. It is not always possible to hypnotise a patient in a short time, or even at the first attempt. Only about six out of every ten people can be hypnotised at the first attempt.

Dr. Milne Bramwell is said to have succeeded perfectly with one lady at the 69th trial, having failed 68 times previously.

There are, moreover, just a few people who seem to be not hypnotisable at all, *and no one can be hypnotised against their will*, except, perhaps, in some rare cases of acute hysteria, or hystero-epilepsy. In these cases the individual is sometimes spontaneously hypnotised by a sudden loud noise, such as a gong, &c. But even when the patient is quite willing to be hypnotised there is a great difference of susceptibility in individuals, and it is quite

[a] Fascination is a method of staring at the patient, and so wearying out his sense of vision. It has sometimes happened in the fascination method that the operator has become hypnotised before the patient.

[b] O'Ferrall's membrane.

erroneous to think that weak people or people who have not strong wills are always the best subjects. On the contrary, strong men and athletes are often the easiest to hypnotise. Lunatics and very old people, as also very young children, are not very susceptible. The best age is from seven to fourteen years, and sex has but little influence. It has been noticed that phthisical patients are often very susceptible.

The first thing that will be noticed in hypnotising a person will be that the pupils contract, probably for accommodation to the object which they are holding near their eyes, and then the pupils pass into a state of dilatation. When the pupils are fully dilated, if the eyes do not of themselves close, passes may be made in front of the patient from above downwards, or the eyelids may be gently closed by the operator, and then pressed upon. The patient will now probably be in the first stage of hypnosis, and can be made experience many strange sensations. In this stage pain, such as toothache or neuralgia, may be quite removed, and the patient wakened up quite well.

The deeper stages may be induced by further suggestions and by passes. These stages are very numerous, but suffice it to say that we may have light sleep, deep sleep, catalepsy, and somnambulism.

Amnesia may or may not exist on waking, according to the stage induced and the suggestions given. Whilst the patient is in the hypnotic state the most extraordinary phenomena may be produced by suggestion. Thus, aphasia, amnesia, analgesia, sensations of heat or cold, exciting or *depressing* emotions, hallucinations and *illusions* of the most varied character may be induced. The peristaltic action of the bowels may be influenced, and diarrhœa has been successfully treated by hypnosis.

De Jong, the famous Dutch hypnotist, could in one patient produce and check borborygmi at will during hypnosis.

Again, many of the senses may be either exalted or entirely suspended by a mere suggestion. Thus, deafness, or blindness, or loss of taste or smell may be induced. As instances of exalted senses we have on record a case where the patient could hear a watch at 35 feet, which in his ordinary state he could hear only at 3 feet. We have another case in which a patient could track anyone in a room, like a dog, by the sense of smell, by being once allowed to smell the glove worn by this person. Again, a hypnotised person, if his vision be exalted, will easily be able to recognise at a considerable distance one sheet of note paper from another by means of the small lines on it, which would be quite indistinguish-

able to our normal eyes. Pain can easily be induced, and a regular blister has been produced by a mere scrap of paper when the suggestion has been made that it was a blister.

Other strange phenomena in connection with hypnotism are :— "Post-hypnotic Suggestion," and the being "*en rapport.*" By post-hypnotic suggestion is meant that after being waked up a patient will frequently act upon some suggestion which has been given to him while under hypnosis. For instance, if the operator tells him that he is to do a certain thing at a certain time after he wakes up he will frequently do it. Being *en rapport* means that the patient will hear and pay attention to no suggestions save those of the operator, or of some person whom the operator has specially introduced to the notice of the patient and commanded the patient to hear and obey.

Let us now consider the various medical ailments in which the use of hypnotism has been advocated in recent years. In hysteria and all allied neuroses, and in fact in *all* neurotic affections, which are very numerous, hypnotism is simply invaluable.

For the relief of various forms of bodily pain and for mental worry it is often most successful. For the cure of perverted moral tendencies the use of hypnotism has also been attended with the most happy and beneficial results. It has also sometimes been used in midwifery, both to bring about a painless delivery and to act beneficially upon the mind of the mother during her pregnancy, for it has long been known that unpleasant sights and mental trouble to the mother will often affect the development of the child "in utero." As instancing this, I may mention the cases which our President related to us at our last meeting in connection with the Armagh railway disaster. Three women who were pregnant received injuries at that disaster, and the child in each case, which was born months after, had a congenital defect corresponding to the nature of its mother's injury. Thus, one woman had her spine injured, and the child was born with a spina bifida. Other such cases that I have myself met with are :—

1. A pregnant woman ran her hand down into a big pot of red jam, believing the jar to be empty. She got a great fright, and her child was born with a red hand.

2. In another case a mouse jumped out of a cupboard and hit the mother on the eyebrow. The child was born with an eyebrow which certainly resembled a mouse in form.

3. In another case the mother got a blow of a beefsteak on her cheek, and the child was born with a large nævus on same cheek.

I heard of another case where a woman's sister had a child born with six fingers, and this so preyed upon her mind that the next child she bore had also six fingers.

Another case I have been told of was that of a young pregnant woman who suddenly received a blow of a bunch of cherries one day in her garden. The child, a girl, which was born some months after, had a nævus on its cheek which closely resembled a bunch of cherries, and my informant went on to state that this cherry nævus got ripe regularly every summer.

To the gynæcologist, too, hypnotism has proved serviceable; and many authenticated cases are on record of dysmenorrhœa and even of amenorrhœa which have been successfully treated by hypnosis after other measures had failed. Ovarian pain, which is sometimes a source of great agony to a woman, has also been entirely removed by hypnotism in cases where the patient had been told that her only chance of recovery lay in operation and the removal of the ovary.

In cases of lunacy, too, hypnotism has been tried, and I believe with some success; but there are many difficulties in the way of its employment, as the patient is usually very suspicious and apt to become violent. Even in such cases as *organic* heart disease hypnotism has done much good in quieting the patient, and in drawing off his attention from the diseased organ, a matter of no small importance in heart affections, as every experienced practitioner will testify.[a] In other diseases, too, a quiet state of mind and body is often of great import, and hypnotism can secure this, acting as a mental opiate as it were.

In the writings of such men as Trousseau, Graves, Sir James Paget, and other eminent physicians, we find great stress always laid upon the influence of the mind over the body, and it is an every-day hospital experience that those patients who are in low spirits and despondent from the start of their illness, run a much worse chance of recovery than those who are high-spirited and hopeful. Many diseases are even produced by mental worry. Thus, we have probably all seen cases of exophthalmic goître or chorea arising in this way; and at the time of the Franco-Prussian war a regular epidemic of exophthalmic goître is said to have occurred amongst the people of Alsace and Lorraine. Trousseau expressly points out how neuralgic and mental worry excites the secretion of neighbouring glands; how intellectual

[a] Dr. William Stokes, in his book on "Diseases of the Heart," lays much stress upon this factor in the cure of disease.

engrossments, anger, &c., will affect the secretion of milk, &c.; how mental disturbances (*e.g.* hysteria) will increase micturition; and how fear may induce diarrhœa. And he impresses upon us the fact that the whole digestive system, including the liver and pancreas, may be largely affected by the mind.

As instancing the effect of the mind on the body, there is a very interesting experiment on record made by Professor "Delbœnf, of Liège." This gentleman hypnotised a patient, and then burnt with caustic his two arms in an exactly similar manner, and to the same extent. He then suggested to the hypnotised man that the right arm would heal without pain, and very quickly, but that the left arm would heal in the ordinary way (*i.e.*, would be painful and take longer). The suggestion was carried out to the letter, and the right arm was well weeks before the left.

Another class of cases in which hypnotism might be useful is in cases of shock—say after a railway accident, where the shock does not come on immediately. Might it not be possible to hypnotise the patient in the interval between the time of the accident and the onset of shock, and then by suitable suggestion to arrest the terrible consequences that sometimes follow such accidents.

In surgery hypnotism has frequently been used as an anæsthetic, and, as mentioned above, Dr. James Esdaile, of Calcutta, has left a record of 250 operations painlessly performed by its aid, while Liston, Velpeau, and Broca, all testify also to its use in this way. Of course since the introduction of chloroform and ether hypnotism is no longer used as an anæsthetic, but one case is quoted in the *British Medical Journal* of Nov. 15th, 1890, where Dr. A. B. Shaw, of St. Louis, had to do a trephining operation for traumatic epilepsy, and where advanced kidney and cardiac disease rendered the use of ether or chloroform dangerous. And in this case hypnotism was resorted to, and the operation painlessly and successfully performed. In cases of insomnia, too, hypnotism is often of the greatest service.

Having now seen the methods of inducing hypnosis, and the phenomena that may be produced by it and also its therapeutic uses, let us for a moment consider the "theories" of what hypnotism really is.

Hypnotism has been said, at different times, to be due to (1) magnetism or some such allied force; (2) electricity; (3) a force which emanates from one being to another, and peculiar to certain individuals; (4) to the mere power of will of one person crushing the will of another; (5) to the power of the devil, &c. We nowadays, however, recognise in hypnotism the expression of

purely physiological phenomena, and as our knowledge of the brain grows so our power of explaining these phenomena is increased. Liébault held that owing to a dulling of consciousness and of sensations, there was a great accumulation of energy in the brain, and that this could then be directed into any one channel by the operator.

A scientist named Tarchanoff—as narrated by Dr. Lloyd Tuckey—has illustrated the powerful action of suggestion in hypnotism by comparing the mind in its waking state to a room into which light is entering from all directions, and the mind in hypnosis to a darkened room. In the first case, the effect is general illumination without undue prominence being given to any one ray. But in the second case, when the room is darkened, if a solitary ray be sent in, this one ray, which before would have passed unnoticed, now shines with exaggerated force and brilliancy. In like manner as a ray of light will pierce the material gloom so will a suggestion in a hypnotised person work its way into the mind with increased power and effect—since there will be no other ideas present to influence it or dispute the field.

Dr. Rudolf Heidenhain, Professor of Physiology in the University of Breslau, first held that hypnosis was due to a contraction of the arteries supplying the brain, and brought about by the sensory stimulus employed to induce the hypnotic condition. He considered hypnosis therefore to be due simply to anæmia of definite parts of the brain. He soon found, however, that this theory could not hold water, as patients in hypnosis are usually flushed in the face, and not pale, as they would be if the arteries were contracted, and, moreover, on experiment he found that nitrite of amyl, which brings much more blood to the brain, even heightened the influence of hypnosis and rendered its induction easier.

Professor Heidenhain's latest theory, and the one which is now most generally accepted for hypnosis, is based upon the physiological doctrine of "inhibitory nervous action." He states that the phenomena of hypnotism are induced by the *inhibition of the activity of the ganglionic cells of the cerebral cortex*, which inhibition is brought about by the prolonged gentle stimulation of the sensory nerves of the face, or of the auditory or optic nerves.

In support of this theory many facts might be adduced, but one example of inhibitory nervous influence with which we are all cognisant from our physiological studies is that displayed by the ganglia of the heart. We all know that if the vagus nerve be stimulated the heart's action will be slowed, and finally with

increased stimulation stopped, whilst stimulation of the sympathetic nerve will quicken the heart. But the fibres from these nerves do not act upon the cardiac muscle, they merely convey impressions to the ganglionic cells of the heart, and it is through these that the heart is influenced. Perhaps the best way of illustrating this inhibitory action of the vagus is to remind you of the fact that a person in a great heat who suddenly drinks off a cup of very cold water is liable to sudden death from inhibition of the heart, through the stimulus transmitted from the stomach by the vagus nerve. Asthma arising from dyspepsia is perhaps another example of the same thing.

This form of nervous influence displayed by the heart is now known to play a very important part in all other forms of nervous action. We also know from our physiology that this peculiar nervous inhibitory action may be induced by certain *sensory* stimuli. For example, if a tight ligature be put round the thigh of a frog which is lying on its back it will lose all power to recover its normal position, or if pressure be made upon the internal organs of a rabbit's abdomen the sensory stimuli thus created will cause paralysis of its hind legs.

A sudden fright may also cause temporary paralysis or unreasoning action. If an elderly lady is suddenly nearly run over in the street she will frequently run wildly in any direction in which the by-standers may shout to her to run, without reasoning, or in fact knowing what she is doing. *So that we arrive at the conclusion that when one set of sensory or recipient cells are in a state of great irritability inhibition of voluntary movements may result, and reflex actions may occur without any true consciousness of their occurrence on the part of the individual in whom they occur.*

This is exactly what takes place in hypnotism. Through the inhibition of activity of certain cerebral ganglia the physiological connection which normally exists between the higher brain (*i.e.*, where will, reason, and consciousness are situated) and the lower brain (*i.e.*, the automatic or reflex brain) are weakened, or for the time being completely cut off. The consequence of this is that any impressions which can be conveyed to this lower brain will produce results without the intervention of the higher brain—that is, will produce simple reflex results without any consciousness, reasoning, or volition on the part of the subject. It may at first sight seem strange that mere suggestion should cause these reflex acts, but it will not seem so strange if we but consider for a moment the way in which in our early nursery days we all learnt

to perform the simplest of our acts. Were we not influenced in everything by the suggestions of those around us, whether those suggestions were given by word of mouth or by their actions? And do we not even in adult life often receive and act upon the suggestions of others without troubling to reason out or analyse those suggestions?

Certainly eight people out of ten whom you meet in the street if you say to them, "It's a fine day," or "It's a gloomy day," will immediately acquiesce, and say, "Yes, indeed it is"—even though they might, if they took time to consider the question, be of quite a contrary opinion.

Again, you are going out, say, and the day looks gloomy. You take up your umbrella and prepare to start, when some friend who is going out with you says: "Oh, don't bring the gamp with you, it's sure not to rain, and it will only be a nuisance having to carry it." How often would you not be influenced by this suggestion, and leave the umbrella at home, even though in your own judgment you thought it might rain? This shows us how ready we are even in our normal state to receive suggestions. This readiness to receive and act upon suggestion is merely heightened in hypnosis, for in this condition the state of childhood is in a way recalled, inasmuch as the physiological functions of the higher brain are in hypnosis suppressed, whereas in childhood they are undeveloped. By some it is held that this ganglionic inhibitory action may be exercised not, as suggested above, on the lower cells of the cerebral cortex, resulting in a cutting off of the higher from the lower brain, but upon the higher cells of the cerebral cortex, so that impressions may reach the higher brain, but that here the cells are in a state of inhibition to receive any impulses from the will of the hypnotised person, and so the reflex actions are produced by impulses passing right through the higher brain as well as the lower, and hence out by the efferent tracts.

There is, perhaps, just one point which it might be well to touch upon at the close of my remarks. The assertion is sometimes made that hypnotism is unjustifiable from a moral point of view, inasmuch as the morals, especially of women, are liable to suffer from its use. Suffice it to say in this connection that men who have devoted their lives to the study of the subject are of an entirely different opinion. Professor Bernheim, after an experience of 10,000 cases, expresses his entire satisfaction of it both morally and physically.

Professor Heidenhain and von Krafft Ebing are of the same

opinion, as also are Drs. Moll, Kingsbury, Lloyd Tuckey, de Jong and a host of others; while Dr. Liébault, after thirty years continuous practice of it, says that he cannot recall a single instance in which he regrets having adopted it.

These facts speak for themselves, and it is worthy of note that those who cry down hypnotism on moral grounds, and are its fiercest antagonists, are invariably those who understand it least, and who have never seen it largely practised. For my own part, I believe that so far from being morally injurious, hypnotism is frequently of the greatest moral good. Many who had been leading the most debauched lives have been brought back to a sense of duty by its aid, and many a drunkard has been reclaimed.

In conclusion, I may say that I am well aware that many points—perhaps points of much interest—have been omitted from my remarks, but inasmuch as my space is limited, and that these points have no direct bearing upon the medical aspect of the question, I have been obliged to pass them over in silence.

For the information contained in the foregoing remarks I am deeply indebted to the works of Drs. Kingsbury and Charles Lloyd Tuckey, of England, and of Dr. Charcot, of Paris.

A CASE OF SYPHILIS WITH NO PRIMARY SORE.

Dr. C. F. Marshall (*Lancet*, May 20, 1899) reports the following case, which was under observation from the first :—"On March 21st a man, aged about 25 years, was sent to me by Dr. Steegmann with a view to the diagnosis of syphilis. He had a roseola rash on the chest, abdomen, and flanks, but no other signs, and no primary sore. In January (two months before) he had contracted gonorrhœa, and feared that he had also got syphilis owing to some inflamed smegma glands exciting his suspicion. From that date up to the present time he was under the observation of Dr. Steegmann and myself, and at no time did any 'primary sore' develop. Later, in March, a typical polymorphous secondary syphilide appeared, followed by injection of the fauces and mucous patches on the prepuce. Writing on syphilis in 1897 Stassano was of opinion that many cases of syphilis occur without a primary chancre, and that a solution of continuity is not necessary for its formation, as shown by the occurrence of chancres on the tonsils, where such solutions of continuity could hardly occur."

PURVEYORS TO H.R.H ICH DIEN THE PRINCE OF WALES

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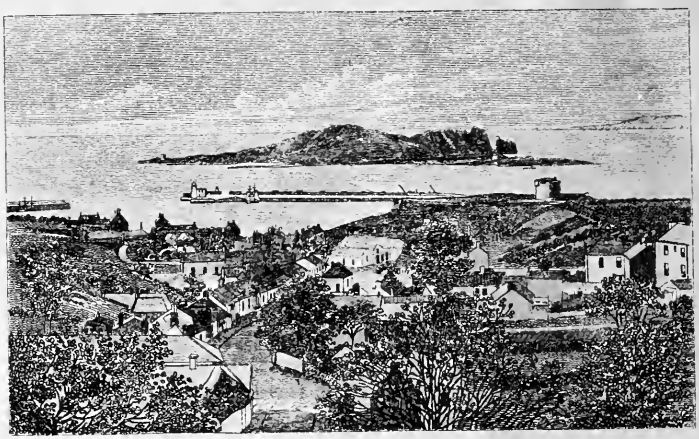

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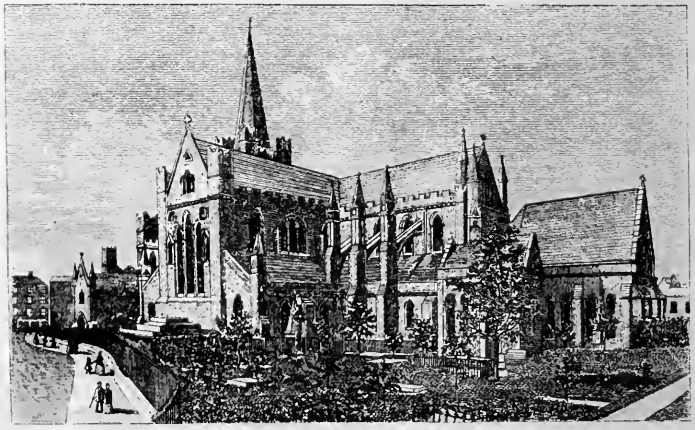

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SECTION OF MEDICINE.

President—J. W. MOORE, M.D. Univ. Dubl.; President of the Royal College of Physicians of Ireland.

Sectional Secretary—R. TRAVERS SMITH, M.D. Univ. Dubl.

Friday, March 10, 1899.

The PRESIDENT in the Chair.

Ulcer of the Œsophagus.

DR. J. A. LINDSAY (Belfast) read notes of a case of above. [They will be found at page 266.]

DR. FINNY said he could not understand how mediastinal emphysema could reach the eyelids, though, of course, there were records of emphysema of the eyelids from rupture of the air passages.

DR. HAYES related the case of a young man dying of rapid general tuberculosis, who two days before death complained, after drinking some milk, of intense pains over the sternum. An autopsy showed the extravasation of milk surrounding the œsophagus, which had two ruptures about half-way down.

DR. COLEMAN referred to the case of a boy who, after swallowing a large fish bone, developed not only pus, but also air in pericardium, due, no doubt, to the fish bone ulcerating through the œsophagus. He believed that the gas in the pericardium was due to decomposition, and this was also a possible explanation of the emphysema which was present in Dr. Lindsay's case.

DR. LINDSAY replied.

Probable Tubercular Meningitis.

DR. LINDSAY also read notes of a case of probable tubercular meningitis. [They will be found at page 268.]

DR. DRURY said that the frequent pulse, regular throughout, the rather high temperature, the absence of strabismus or any form of paralysis, and the involuntary passage of the evacuations, led him to believe that it was not tubercular meningitis. He suggested

that it was a case of rheumatic pleurisy with some rheumatic meningitis.

Dr. Finny said he had very great doubts that it was a case of tubercular meningitis. He thought that many of the cases condemned as tubercular meningitis may really be ordinary meningitis.

Dr. Knott said that tubercular meningitis as a primary and uncomplicated disease occurred very rarely. In children it was nearly always a part of general tuberculosis.

Dr. R. Travers Smith asked whether the choroid coat of the eye had been examined for the presence of tubercle.

The President thought as cases of tubercular peritonitis could recover without operation so might tubercular meningitis. He suggested that they should try the tuberculin test in these cases, but he believed that both pleural effusion and the head symptoms were possibly due to the micro-organisms of pneumonia, which sometimes produced a very dangerous form of meningitis.

Dr. Lindsay, in reply, said that the President's suggestion of pneumonia had also occurred to him. Tubercles were not found in the choroid, but the patient was so restless that the examination of the eye was accomplished with difficulty.

Non-febrile Pneumonia.

Dr. Lindsay also read notes of a case of non-febrile pneumonia. [They will be found at page 270.]

The President said there was nothing new in the apyrexia. The new doctrine about the production of pyrexia was that it depended upon the effect on the central nervous system of the albumoses developed by the action of the poison, whereas the ptomaïns produced the other head symptoms. It was possible that the action of the albumoses had an opposite effect in certain cases, and produced collapse. He considered that the hypodermic administration of strychnin was one of the best remedies in the collapse of all fevers.

Dr. Cosgrave mentioned a case of pneumonia in which the temperature remained subnormal throughout. He had tried hypodermics of strychnin, but they had had no effect.

Dr. Lindsay, in reply, said he always gave strychnin in these cases.

Morphinomania.

Dr. H. C. Drury read a paper on this subject. [It will be found at page 321.]

Dr. Dawson, Dr. Cosgrave and the President discussed it.

The Section then adjourned.

SANITARY AND METEOROLOGICAL NOTES.

Compiled by J. W. Moore, B.A., M.D. Univ. Dubl.;
P.R.C.P.I.; F. R. Met. Soc.;
Diplomate in State Medicine and ex-Sch. Trin. Coll. Dubl.

Vital Statistics

For four Weeks ending Saturday, April 22, 1899.

The deaths registered in each of the four weeks in the twenty-three principal Town Districts of Ireland, alphabetically arranged, corresponded to the following annual rates per 1,000 :—

Towns	Weeks ending April 1	April 8	April 15	April 22	Average Rate for 4 weeks	Towns	Weeks ending April 1	April 8	April 15	April 22	Average Rate for 4 weeks
23 Town Districts	29·0	27·3	25·4	23·7	26·3	Limerick -	43·5	16·8	18·2	8·4	21·7
Armagh -	14·3	28·5	21·4	7·1	17·8	Lisburn -	12·8	38·3	17·0	17·0	21·3
Ballymena	45·1	39·5	28·2	73·3	46·5	Londonderry	25·1	11·0	29·8	23·6	22·4
Belfast -	29·5	25·9	21·5	20·3	24·3	Lurgan -	9·1	22·8	13·7	18·2	16·0
Carrickfergus	23·4	5·8	11·7	5·8	11·7	Newry -	16·1	16·1	32·2	12·1	19·1
Clonmel -	29·2	39·0	43·8	9·7	30·4	Newtownards	28·3	22·7	17·0	17·0	21·3
Cork -	27·0	29·8	16·6	21·5	23·7	Portadown -	24·7	12·4	12·4	49·5	24·8
Drogheda -	22·8	19·0	7·6	7·6	14·3	Queenstown	11·5	11·5	23·0	5·7	12·9
Dublin - (Reg. Area)	30·4	31·9	32·3	29·7	31·1	Sligo -	25·4	15·2	20·3	10·2	17·8
d alk -	8·4	33·5	29·3	37·7	27·2	Tralee -	89·5	33·6	16·8	33·6	43·9
Galway -	11·3	34·0	18·9	41·5	26·4	Waterford -	29·8	29·8	31·8	27·9	29·8
Kilkenny -	33·0	18·9	56·6	18·9	31·8	Wexford -	18·1	22·6	22·6	18·1	20·4

In the week ending Saturday, April 22, 1899, the mortality in thirty-three large English towns, including London (in which the rate was 17·2), was equal to an average annual death-rate of 18·3 per 1,000 persons living. The average rate for eight principal towns of Scotland was 18·1 per 1,000. In Glasgow the rate was 19·3. In Edinburgh it was 17·2.

The average annual death-rate represented by the deaths registered during the same week in the Dublin Registration Area and in the twenty-two principal provincial Urban Districts of Ireland was 23·7 per 1,000 of their aggregate population, which, for the purpose of this return, is estimated at 1,053,188.

The deaths from the principal zymotic diseases in the twenty-three districts were equal to an annual rate of 2·3 per 1,000, the rates varying from 0·0 in fourteen of the districts to 28·0 in Tralee—the 6 deaths from all causes registered in that district comprising 5 from measles. Among the 136 deaths from all causes registered in Belfast are 6 from measles, 2 from scarlatina, 6 from whooping-cough, 2 from diphtheria, one from enteric fever, and 2 from diarrhœa. The 31 deaths in Cork comprise 2 from whooping-cough and one from diarrhœa. Among the 14 deaths in Waterford are 2 from measles and one from simple continued fever. The 9 deaths in Dundalk comprise 3 from whooping-cough.

In the Dublin Registration Area the births registered during the week ending April 22 amounted to 210—105 boys and 105 girls; and the deaths to 202—105 males and 97 females.

The deaths, which are 21 over the average number for the corresponding week of the last ten years, represent an annual rate of mortality of 30·1 in every 1,000 of the population. Omitting the deaths (numbering 3) of persons admitted into public institutions from localities outside the Area, the rate was 29·7 per 1,000. During the sixteen weeks ending with Saturday, April 22, the death-rate averaged 30·1, and was 1·7 under the mean rate in the corresponding portions of the ten years 1889-1898.

The number of deaths from zymotic diseases registered during the week was 19, being 3 below the average for the corresponding week of the last ten years, and 6 under the number for the previous week. The 19 deaths comprise one from scarlet fever (scarlatina), 7 from influenza and its complications, 3 from whooping-cough, 2 from diphtheria, 2 from enteric fever, and 2 from diarrhœa.

The weekly number of cases of scarlatina admitted to hospital, which had risen from 10 in the week ended April 8, to 15 in the following week, fell to 9. Eight scarlatina patients were discharged, and 72 remained under treatment on Saturday, April 22, being one over the number in hospital on the previous Saturday. There were also 19 convalescents under treatment at Beneavin, Glasnevin, the Convalescent Home of Cork-street Fever Hospital.

Six cases of enteric fever were admitted to hospital, or 6 under the number in the preceding week, and one under the admissions in the

week ended April 8. Seventeen patients were discharged during the week, 2 died, and 55 remained under treatment on Saturday, being 13 under the number in hospital at the close of the preceding week.

The number of cases of diphtheria admitted to hospital was 6, being 3 under the admissions in the preceding week, but 4 over the number admitted during the week ended April 8. Three patients were discharged during the week, and 32 remained under treatment on Saturday, being 3 over the number in hospital at the close of the preceding week.

The deaths from diseases of the respiratory system registered during the week amounted to 57, being 20 in excess of the average for the corresponding week of the last ten years, and 14 over the number for the previous week. They comprise 33 from bronchitis, 21 from pneumonia, and 2 from croup.

Meteorology.

Abstract of Observations made in the City of Dublin, Lat. 53° 20′ N., Long. 6° 15′ W., for the Month of April, 1899.

Mean Height of Barometer,	29·796 inches.
Maximal Height of Barometer (22nd, 9 a.m.),	30·257 „
Minimal Height of Barometer (13th, 9 a.m.),	28·965 „
Mean Dry-bulb Temperature,	47·3°.
Mean Wet-bulb Temperature,	44·2°.
Mean Dew-point Temperature,	40·7°.
Mean Elastic Force (Tension) of Aqueous Vapour,	·256 inch.
Mean Humidity,	78·5 per cent.
Highest Temperature in Shade (on 28th),	64·0°.
Lowest Temperature in Shade (on 16th),	35·1°.
Lowest Temperature on Grass (Radiation) (on 18th),	30·0°.
Mean Amount of Cloud,	64·0 per cent.
Rainfall (on 20 days),	1·995 inches.
Greatest Daily Rainfall (on 24th),	·382 inch.
General Directions of Wind,	N.W., W., S.W.

Remarks.

April, 1899, proved a changeable, showery and cloudy month. The wind was particularly variable in direction and force. A prolonged series of gales was experienced from the 4th to the 8th inclusive. There was a cold spell from the 13th to the 19th. As in April, 1898, at the close many forest trees were in full leaf.

In Dublin the arithmetical mean temperature (48·6°) was 0·9° above the average (47·7°) ; the mean dry-bulb readings at 9 a.m. and 9 p.m. were 47·3°. In the thirty-four years ending with 1898, April was coldest in 1879 (the cold year) (M. T.=44·5°), and warmest in 1893 (M. T.=51·4°). The month of April, 1893, was the warmest for at least 30 years.

The mean height of the barometer was 29·796 inches, or 0·054 inch below the average value for April—namely, 29·850 inches. The mercury rose to 30·257 inches at 9 a.m. of the 22nd, and fell to 28·965 inches at 9 a.m. of the 13th. The observed range of atmospheric pressure was, therefore, 1·292 inches.

The mean temperature deduced from daily readings of the dry-bulb thermometer at 9 a.m. and 9 p.m. was 47·3°, or 3·9° above the value for March, 1899. Using the formula, *Mean Temp.=Min.* + (*Max.* — *min.* × ·476), the value is 48·3°, or 0·9° above the average mean temperature for April, calculated in the same way, in the twenty-five years, 1865–89, inclusive (47·4°). The arithmetical mean of the maximal and minimal readings was 48·6°, compared with a twenty-five years' (1865–1889, inclusive) average of 47·7°. On the 28th the thermometer in the screen rose to 64·0°—wind, S.S.W.; on the 16th the temperature fell to 35·1°—wind, N.N.E. The minimum on the grass was 30° on the 18th.

The rainfall was 1·995 inches, distributed over 20 days. The average rainfall for April in the twenty-five years, 1865–89, inclusive, was 2·055 inches, and the average number of rainy days was 15·2. The rainfall, therefore, was slightly below, whereas the rainy days were much above, the average. In 1877 the rainfall in April was very large—4·707 inches on 21 days. On the other hand, in 1873, only ·498 inch was measured on 8 days. In 1898, 2·666 inches fell on 16 days.

Fog was observed on the 1st, 9th, 24th, and 27th. High winds were noted on 10 days, reaching the force of a gale on the 4th, 5th, 6th, 7th, and 8th. Hail fell on the 8th, 14th, and 17th ; sleet on the 17th. The temperature rose to or above 60° in the screen on 5 days. It thrice failed to reach 50° (on the 15th, 16th, and 17th). It never fell to 32° in the screen, but on 7 nights it fell to or below 32° on the grass. The mean lowest temperature on the grass was 37·8°, compared with 40·2° in 1898, 37·7° in 1897, 40·6° in 1896, 37·8° in 1895, 40·0° in 1894, 38·2° in 1893, 32·4° in 1892, 34·1° in 1891 and 1890, 34·4° in 1889, 34·6° in 1888, and 31·6° in 1887. Solar halos were seen on the 3rd, 16th, 18th, and 19th ; lunar halos on the 18th and 19th. Slight thunder and lightning occurred on the 25th.

The rainfall in Dublin during the four months ending April 30th amounted to 7·557 inches on 71 days, compared with 7·236 inches on 64 days in 1898, 9·554 inches on 79 days in 1897, 5·781 inches on 63 days in 1896, 10·233 inches on 65 days in 1895, 9·151 inches on 73 days in 1894, 6·242 inches on 56 days in 1893, 5·922 inches on 61 days in 1892, only 3·203 inches on 46 days in 1891, and a twenty-five years' average of 8·466 inches on 66·2 days.

At Knockdolian, Greystones, Co. Wicklow, the rainfall amounted to 2·690 inches on 19 days. The heaviest falls in 24 hours were ·550 inch on the 12th and ·565 inch on the 13th. The total rainfall in 1899, up to April 30th, was 12·380 inches on 70 days, compared with 8·890 inches on 56 days in 1898, 13·080 inches on 80 days in 1897, 5·686 inches on 50 days in 1896, 12·570 inches on 54 days in 1895, 12·456 inches on 70 days in 1894, and 8·530 inches on 54 days in 1893.

At Cloneevin, Killiney, Co. Dublin, 2·07 inches of rain fell on 20 days. The maximal fall in 24 hours was ·48 inch on the 13th. The average rainfall in April of the fourteen years, 1885–98, was 1·977 inches on 12·7 days. Since January 1, 1899, 9·02 inches of rain fell at this station on 65 days, compared with 7·74 inches on 61 days in 1898, 10·36 inches on 83 days in 1897, 5·27 inches on 55 days in 1896, 11·28 inches on 66 days in 1895, 9·09 inches on 74 days in 1894, and 6·94 inches on 57 days in 1893.

At the National Hospital for Consumption, Newcastle, Co. Wicklow, the rainfall was 2·722 inches on 19 days, compared with 4·441 inches on 15 days in 1898, and 3·406 inches on 19 days in 1897. On the 12th ·463 inch was measured, and on the 13th ·510 inch. The maximal temperature in the shade was 61·2° on the 3rd. The minimal temperature in the screen was 33·7° on the 18th. At this station 12·651 inches of rain fell on 67 days up to April 30th, compared with 9·208 inches on 55 days in the corresponding period of 1898, and 13·492 inches on 76 days in the first four months of 1897.

GLANDERS IN BELFAST.

Between September, 1898, and the present date several cases of glanders having occurred, especially among horses owned by car-drivers using the same "hazard," the Veterinary Department of the Privy Council, Dublin, have instructed the authorities to disinfect the stands, and if necessary to close the drinking fountains used by animals, and to take any other required precautions.—*Lancet*, May 20, 1899.

NEW PREPARATIONS AND SCIENTIFIC INVENTIONS.

Artificial Effervescent Mineral Water Salts.

Messrs. Burroughs, Wellcome, & Co., of Snow-hill Buildings, London, E.C., have prepared a new series of "Tabloid" effervescent artificial mineral water salts, which form pleasant effervescent draughts, one in a given quantity of water being equal in strength to the same volume of Carlsbad, Kissingen, Vichy, or natural seltzer water. They are far more compact than ordinary salts or bottled waters, and render possible a continuous course of mineral water treatment in all lands and under all circumstances.

By prescribing them in conjunction with a suitable dietary the physician is enabled to give the benefit of a course of spa treatment to those to whom it would otherwise be a difficult measure. At the same time the patient is constantly under the care of his own medical adviser.

The high standard of purity of constituents and accuracy of dosage characteristic of "Tabloid" drugs is maintained in these compressed mineral water salts.

"*Tabloid*" *Carlsbad Mineral Water Salt.*—One in two ounces of water represents an equal volume of Carlsbad Sprudel water in all its essential constituents; one, two or three, as required, should be dissolved in a tumbler of hot water and sipped slowly while dressing, or they may be placed in two-thirds of a tumbler of cold water and taken as the effervescence subsides.

"*Tabloid*" *Kissingen Mineral Water.*—One, dissolved in two ounces of water, represents an equal quantity of Kissingen water in all its essential constituents.

"*Tabloid*" *Seltzer Mineral Water Salt.*—Contains all the essential constituents of seltzer mineral water in correct proportion. One, or two, in half a tumbler of water, forms an agreeable draught, possessing the refrigerant, tonic, diuretic, and aperient properties of the waters from the cold springs at Seltz (Nassau).

"*Tabloid*" *Vichy Mineral Water Salt.*—One dissolved in two ounces of water represents an equal quantity of Vichy water (Grand Grille Spring) in all its essential constituents.

"*Tabloid*" *Vichy Salt with Lithium.*—Is a valuable antilithic. Each contains lithium citrate, gr. 1, in addition to all the essential constituents of Vichy water.

The "Tabloid" effervescent mineral water salts are supplied in tubes of 25.

INDEX

TO THE ONE HUNDRED AND SEVENTH VOLUME.

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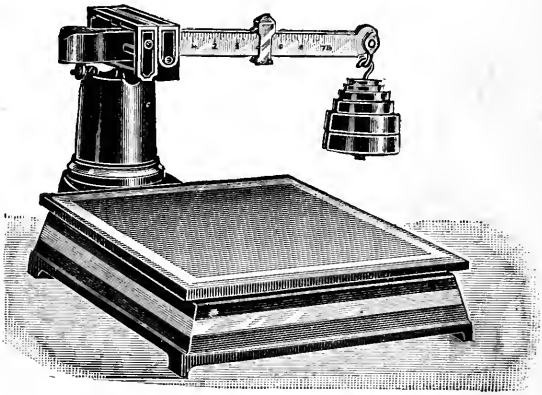

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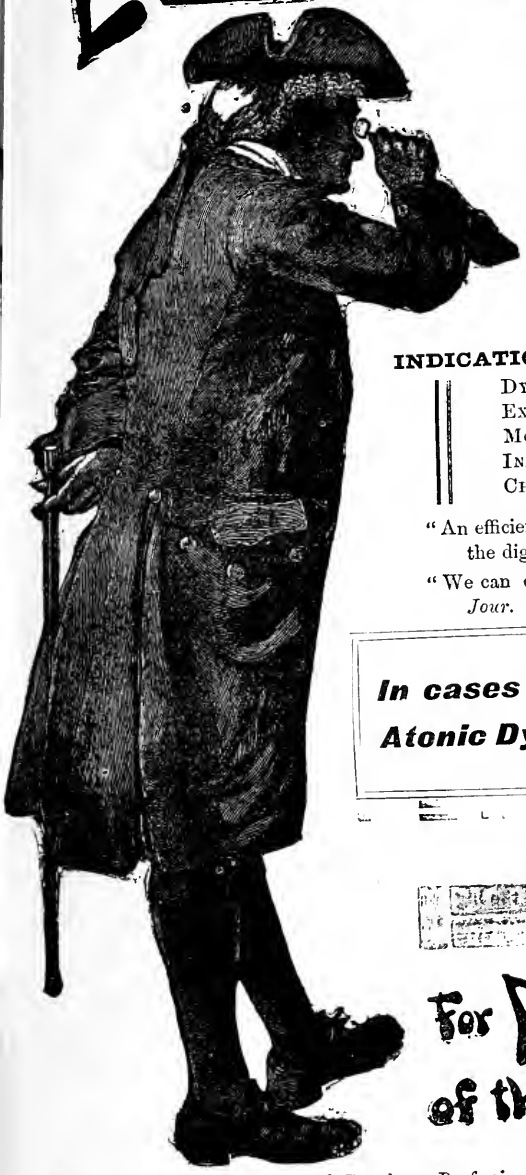
Ft. in.	St. lb.	St. lb.	Years.	St. lb.	St. lb.
4 10	...	7 0	5	3 8	2 12
4 11	...	7 4	6	3 12	3 1
5 0	...	7 7	7	4 1	3 6
5 1	...	7 12	8	4 4	3 10
5 2	9 0	8 2	9	4 9	4 1
5 3	9 7	8 9	10	4 13	4 6
5 4	9 13	9 2	11	5 3	4 13
5 5	10 2	9 9	12	5 9	5 8
5 6	10 5	9 13	13	6 0	6 5
5 7	10 8	10 8	14	6 8	7 0
5 8	11 1	11 4	15	7 5	7 8
5 9	11 8	...	...	...	...
5 10	12 1	...	..	...	...
5 11	12 6	...	...	...	...
6 0	12 10	...	...	...	...

LACTOPEPTINE



FORMULA.

Sugar of Milk.....	40	ounces.
Pepsin (Pure).....	8	,,
Pancreatine (Pure)	6	,,
Veg. Ptyalin or Diastase.....	4	drachms.
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"Scotland, 25th Sept., 1896.

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